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PHYSICS AND HYDRAULICS OF THE MISSISSIPPI.

REPLY TO CERTAIN CRITICISMS MADE BY DR. HAGEN, DIRECTOR-GENERAL OF PUBLIC WORKS, PRUSSIA.

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Written for Van Nostrand's Engineering Magazine.

It is eminently proper that any book claiming to embody the results of extended and original investigation in an important branch of science should be subjected to very careful scrutiny before its conclusions are accepted. To afford every possible facility for such a study of the Report upon the Physics and Hydraulics of the Mississippi, its authors were careful to publish the data, in detail, so tabulated and illustrated by diagrams that any critic of moderate industry might reach the bed-rock upon which the conclusions were founded.

The work was translated into the principal modern languages of Europe, and has received marked attention from many eminent hydraulic engineers and scientists. The tone of criticism from this class of writers has generally been favorable, and has uniformly been courteous, with one distinguished exception—Dr. G. Hagen, Director General of Public Works in Prussia.

This gentleman, one of the most widely known hydraulicians of Germany, had published an extensive work on hydraulics in 1844; but it happened not to be accessible to the writers of The Physics and Hydraulics of the Mississippi, and, in the hurry of finishing the Report at the outbreak of the late civil war, no reference to it was made in their somewhat full historical resumé of the subject.

Mr. Heinr. Grebenau, Royal Bavarian Officer of Public Works, who, in 1867, published a translation of The Physics and Hydraulics of the Mississippi into German, with original notes and new material, made use of expressions of which the following are samples: "This theory, which makes an undoubted epoch in the history of hydraulics, dispels the darkness which even the latest hydraulicians such as Woltmann, Brünings, Eytelwein, Funk, and Hagen among the Germans, and Dubuat, D'Aubuisson, de Prony, Dupuit, and others among the French, have tried in vain to clear away." "While the older hydraulicians made known the results of their velocity measurements, which were often made with defective instruments, and thus rendered a service to science and practice, even now Hagen

gives neither the interesting observations made by him, nor, generally, any of his water measurements. Under these circumstances it is evident how the German learning and profoundness could be surpassed by the spirit of enterprise and perseverance of the Americans."

Dr. Hagen himself, referring to variations of velocity below the surface of flowing water, writes: "In this connection, the well known authors (of the Physics and Hydraulics of the Mississippi) propounded certain theories which found special favor with the translator of their work into German, and which in our country were so enthusiastically received that the danger of their general acceptance seemed imminent. I was, therefore, prompted to prove how little the observations upon which the theories were based are calculated to sustain them."

Under such circumstances, apparently slighted by us in the original work, and his own efforts contrasted unfavorably with ours by the German translator, himself a hydraulic engineer of eminence, the ordinary impulses of human nature might be expected to introduce some bitterness into the manner of presenting this "proof." We had, however, a right to expect that no *misrepresentation* should occur. Whether it did, or did not, will appear from the following facts.

Dr. Hagen, after describing the method employed by us for deducing the law governing the change in velocity from surface to bottom, brings the first specific charge in the following language. "The places on the river where the observations were made, or the different base lines were measured, are not sufficiently described. The reader is not informed whether the depths recorded extend over a greater length, or whether they are to be considered as limited depressions of the river bed only. The series of means given in the work refer twenty-seven times to the spot where the *prime base* was measured. This place is situated near Carrollton immediately below a very sharp bend of the Mississippi, as shown by a very small map on plate III, figure 2. The same map shows also that the *race-course base*, to which four series of means are referred, is situated a little farther down the river on a straight part of its course. Of the three other places of observation the *Locks base*, the *Baton Rouge upper base*, and the *Baton Rouge lower base*, no information whatever is given. The width of the river at those places is nowhere indicated, although the distance of the buoy from the base is given in every case. From the small map alluded to, the Mississippi would appear to be about 2,200 feet wide at the first base, and about 2,500 feet at the second."

The facts are, that (in Appendix C) every possible detail respecting all these sections is presented, viz: a complete list of distances and corresponding soundings extending entirely across the river; the nature of the bottom; the high-water and low-water dimensions of cross-section, including the area, width, and wetted perimeter; and even the dates when the soundings were made. To render it certain that no reader should fail to discover every desired detail, his attention is especially invited on the small map to which Dr. Hagen refers, to Appendix B which gives the gauge reading on the days when the velocity measurements were made, and thus enables him to study changes in the curve in connection with the changes produced by the oscillation of the river. The *Locks base* is distinctly laid down on this map, although Dr. Hagen asserts that it is not. Plate II shows so clearly that Baton Rouge is on a straight part of the river, with no bend in the vicinity, that a small local map was considered unnecessary; but every detail respecting the sections there appears in the Appendices. Apparently Dr. Hagen was too much absorbed in searching for "proof" to give much attention to the text.

He continues as follows: "The velocities are given to within one ten-thousandth of a foot, or to within $\frac{1}{10}$ of a line. Such exaggerations may possibly fascinate those readers who are not aware of all the circumstances connected therewith; but, in general, they only show that the investigations were carried on without regard to the degree of accuracy attainable."

To a practical investigator the true and very prosaic explanation of the reason why we published so many decimals

might be expected to suggest itself. Many of the quantities computed in the Report were so large that logarithmic tables reading to seven places, and giving results without interpolation in five figures, were uniformly employed. With the velocities now under consideration, this gives four decimal places—evidently more than are needful to represent the observations, but which can do no harm, except possibly to entrap a searcher for "proof."

Dr. Hagen next proceeds to analyze very closely the table exhibiting the results of the subsurface velocity measurements; and he records several of his discoveries in a manner calculated to leave on the mind of the reader the impression that they have some significance injurious to the work. These suggested, but not specified, charges are best answered by a short statement of facts.

In general, no attempt was made to record the time of transit of a single float closer than to the nearest second; but many of these special sub-surface velocity observations were recorded with a stop-watch reading to quarter seconds. In combining several series of observations, decimals of seconds were of course always retained; but when only two floats had passed, this was sometimes neglected, the mean expressed in the nearest second being considered sufficiently exact. These simple facts give a full explanation of all Dr. Hagen's mysterious discoveries in arithmetic, except one which is thus stated. "Finally it is to be mentioned that in Group II, in 5 series, or in 36 sets of observations, the entire depth of the river was only 65 feet, while the velocities are reported as having been measured at a depth of 66 feet."

The explanation of this paradox consists of two elements. First, in two of the five series, Dr. Hagen is in error when he asserts that the Report gives any velocity as "measured" at sixty-six feet below the surface. The figures are in "old style type," and facing the first page of the report is a conspicuous note calling attention to the fact that this indicates interpolation. Second, Dr. Hagen places a forced construction on our language when he asserts that the entire depth of the river was only 65 feet. The Report states that it was "about 70 feet," which was actually the case. If Dr. Hagen had as closely inspected the figures in the column headed "depth" as he has done those in some of the others, he would have perceived that they are always expressed in multiples of 5 feet; and hence are evidently given as approximate. Any depth from $62\frac{1}{2}$ to $67\frac{1}{2}$ feet would appear as 65 feet, and Dr. Hagen has, therefore, no fair grounds for his criticism. As a matter of fact, however, upon referring to the original note books and diagrams used in the analysis, it is discovered that the depths printed as 65 feet in this table should have been 75 feet; and that the mistake occurred in transcribing the records for the press. The correct number was used in the analysis.

Having now driven in Dr. Hagen's skirmishers, we reach the main body of his attack. Without attempting to quote him in detail, it is believed that his views are fairly represented in the following summary. Dr. Hagen considers that the the actual accordance between our grand mean curve, representing 222 observations at each point, and a formula, which we deduced from it to represent the law of change below the surface, "transcends so much every conceivable degree of accuracy, that instead of confirming the result it is rather calculated to render it suspicious." Here, then, a vital issue, the accuracy of our mathematical deductions from our data, is directly raised by Dr. Hagen, and he adduces two lines of argument to convince his readers that they are untrustworthy. The first is what he claims to be a statement of our method of reduction, with criticisms thereon; and the second is a theoretical computation, upon assumed data, showing what accordance should exist between measurements and theory, and that in this case the probable limit was exceeded.

It is not permitted to assume that a gentleman of Dr. Hagen's official position, even when engaged in the patriotic duty of preventing his country from being inundated by error, should deliberately misrepresent plain statements of an official document published by a foreign government. That he has done so, and in the grossest manner, is undeniable; and we therefore assume that eagerness for "proofs," and want of familiarity

with the English language, combined to produce so surprising a result.

The apparent weight of Dr. Hagen's first line of argument is due to its confusing and misleading the reader as to what was actually done in reducing the observations. The shortest way to refute it, is to explain exactly the several steps; which can hardly be done in clearer language than that of the original Report.

"To counteract as far as possible any effect of change in velocity during the observations, the order of observing at different depths was constantly varied. Sometimes a series of observations consisted of one at each depth from surface to bottom, or bottom to surface. Sometimes many observations were made consecutively at each depth. Sometimes floats were started near the surface and near the bottom, and the distances between the planes were successively increased until the mid-depth was reached. In fine, every effort was made to avoid and eliminate error. The first steps toward deducing the law from the observations were therefore very simple.

"As floats are compelled to pass through nearly the same paths when starting from a fixed station, and are consequently unaffected by the change in velocity due to difference in distance from the banks, the principle was adopted of depending entirely upon the elaborated sets of observations from anchored boats. All the observations of each set being thus confined to nearly the same vertical plane, one great cause of error was practically eliminated. From the position of the boat, found by triangulation, the recorded gauge reading and the known depths of the different parts of the river section, the depth of water in each vertical plane was readily determined. The velocity of each float was deduced from the recorded seconds of transit past the base line, and a mean taken of all the observations at each depth for the true velocity at that depth."

It will be noticed that up to this point not one word has been said about any discussion of "single sets of observations" prior to this arithmetical grouping of the data. In truth no such subdivision of these observations into "single sets" was possible, as is apparent when the reader remembers the continual variation in sequence of the observations at the different depths. There was no way to deduce primary curves of observation at any anchorage and date, except to take a "mean of all the observations at each depth for the true velocity at that depth."

But Dr. Hagen conveys to the reader a totally different idea, by transposing a subsequent process of the reduction backward, and pretending that it was applied to imaginary "single sets." He thus represents that the primary figures in the text are not the simple means of observation, but figures derived by an arbitrary process from such original sets; and, hence, that they are vitiated by our preconceived ideas as to what the curve ought to be. Any such view is absolutely false, and is warranted by no line or letter of the Report; and yet Dr. Hagen's whole first argument is based upon it, as is seen by the following extract from his paper:

"After the conviction had been arrived at, that below the surface the velocities first increase with the depth and then decrease until the bottom is reached, every single set of observations was graphically represented, and this was done on so large a scale as to admit of the reading of one thousandth of a foot of velocity. The drawing, therefore, was far more accurate than the observations which were represented by it. Between the points thus obtained, a curve was drawn which satisfied the conditions mentioned above, and which at the same time exhibited the closest possible connection with the observations.

"The reader cannot repeat this operation for himself, since only one set of observations is given which is entirely free from any combinations; and this particular set can indeed be made to furnish approximately a curve of the character mentioned above. All the other sets, however, appear in combinations, and contain the means of these combinations only."

Continuing this misrepresentation, Dr. Hagen selects the three worst of these *primary*, not *combined* curves, and concludes: "These combinations show so great anomalies as to impress us with the arbitrary character of their graphical representation. If the law of the curve

had been known previously, then of course its elements might have been computed by the method of least squares; but in that case it might also have appeared that some other curve, or even a straight line, was the more probable expression of the law than the curve originally introduced."

He does not state, what is the fact, that we decided that the number of observations from which these primary curves were derived was not sufficient to cancel abnormal influences; and that we made no attempt to discuss them but concluded, "that some combination of curves was necessary to reconcile discrepancies of observation."

How the combination was effected by us is explained by the following extract: "The first method adopted was to combine all curves of observation where neither the depth of water nor the velocity of the river varied materially. This was done by taking a mean of the velocities of all the floats at each depth, each set of observations thus receiving a weight proportioned to its number of observations at each point. When observations were wanting at any depth, careful interpolations were made from the plotted curve. The resulting mean curves are exhibited on Plate XI, Figures 1, 3, 10, 2, 4, 9; the numbers being shown in the following tables."

These tables, six in number, represent on the horizontal lines the original primary means obtained arithmetically from the several floats. When observations at any depth are wanting, the interpolations adopted by the authors are given, printed in "old style" figures, so that any critic can revise them. The foot line of each table gives the arithmetical mean of the primary means, and represents the combined curve for that particular depth and velocity.

We now come to our final process, which Dr. Hagen has misrepresented by pretending that it was applied to his imaginary "single sets." It will hardly be denied that the process was legitimate and necessary; and every facility for repeating it in detail was extended to the critic by the tables published in the Report.

"These curves," as the Report states, "at once indicate the existence of law, although the discrepancies are too great to permit the deduction of any algebraic expression for it. It is evident, however, that the velocity differs very little at different depths; that it at first increases as the depth is increased; that the point of maximum velocity is found at a very variable depth below the surface; and that the degree of curvature of the curve varies with the stage of the river.

"It is manifest that some further combination is necessary in order to eliminate the effect of disturbing causes. Since the absolute depths differ, this can only be done by combining the velocities at proportional depths, leaving the correctness of this principle of combination to be eventually tested by the application to each individual curve of the law thus deduced. The method adopted for this combination was to plot the mean curves on a scale so distorted that thousandths of a foot of velocity were readily distinguished." In other words, the arithmetical means of the observed velocities were plotted at their respective depths, and connected by lines which, it will be found, in nearly every instance were *right lines*—the only exceptions being when a decided general change in curvature above and below suggested a slightly curved line. "The entire depth was then divided into ten equal parts. Horizontal lines were drawn, and the velocities at their points of cutting the curves noted. These numbers were the most correct interpolations that could be made for the velocity at each *tenth of depth*, and they were next combined in the ratio of the number of observations. The points inclosed by circles in Fig. 16, Plate XI, exhibit the mean points thus determined, the grand mean of all the observations from anchored boats. They are plotted from the first column of the next table. Each point is fixed by 222 observations; enough, as the result proves, to eliminate irregularities and to reveal the law governing the transmission of resistance through the fluid."

Having thus shown that Dr. Hagen's first line of argument rests solely upon his misrepresentations of what we did, and is not applicable to what we actually did, we proceed to notice his second argument. He says: "The question remains to be answered, How is the demonstrated agreement of the new law with the observations to be accounted for?

That the errors of observation should have adjusted themselves so completely by mere accident cannot well be assumed, since the probability of such a self-adjustment is altogether too small."

He then proceeds, upon assumed data, to compute what this probability is, and arrives at the conclusion that it is one in thirty billions. He then proceeds:

"This definite form of the phenomenon, however, *has* occurred, and from the small probability of its causation by *other* agencies, we may infer the *probability* of its actual causes. Such might, for instance, be the intentional selection of some observations in preference to others which, not agreeing with the preconceived law, were rejected as inaccurate or erroneous. This cause is in itself by no means improbable. Persons who are not accustomed to scientific exactness sometimes believe that such a proceeding is admissible and entirely correct. In the present case the measurements were of an official character and were carried on under a kind of formal control; it might therefore not have been an easy matter to reject observations as erroneous which, when made, were not doubted.

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"There is, however, another and indeed a very natural explanation of this agreement. Let the reader try to establish a graphical connection between the series given in the above drawings and curves of such character as described above, and he will see at once that this is quite an arbitrary process. The most various curves are equally admissible; their course may be arbitrarily changed either in whole or in detail without the introduction of an error. Thus an easy method was obtained to establish, first a connection between the observations and any curve which had been previously selected, and then to reduce quite arbitrarily the errors still remaining. There would have been no difficulty in drawing the curves so as to make the means agree with the computation to within seven decimals.

"The young student of hydraulics is sometimes compelled to accept certain theorems as true and proven which, to say the least, are still doubtful; but he has as yet never been expected to receive devoutly a demonstration like this, and to regard it as a progress of science."

An argument of this nature is so extraordinary, that it is not easy to reply to it with dignified composure. We will simply say that there was no "intentional selection of some observations in preference to others." Every record was admitted and published. Also that there was no use whatever of the arbitrary process which Dr. Hagen has imagined, and, without any grounds for so doing, has asserted that we did use. No mathematician will dispute that in combining such curves interpolation cannot be avoided where points are missing. To guard against any possible misconception, we indicated in every instance such interpolations in the tables by "old style" figures. Not a single interpolation was made which is not thus submitted for the revision of the critic. Out of a total of 369 points of the primary curves, only fifty are interpolations; and the vast majority of these occur in sensibly straight portions where a simple mean can be and was used. If Dr. Hagen can point out any sensible change which can be made in our grand mean curve of observations by correcting errors in these interpolations, he will succeed in reducing the "incredible" accordance between it and our theory. If not, he must revise his own computations as to probable error. These interpolations, all plainly indicated in the text, are the only points open to discussion; everything else is direct measurement.

As to publishing the observations in the minute detail which Dr. Hagen regards as essential, it need only be remarked that the cost would have been quite beyond the amount of funds available. To have done so for this single subject of change of velocity below the surface, would have added many pages of figures. The corresponding observations at other points, and those for the change of velocity from shore to shore, should upon the same grounds be published; and so should also the diagrams showing the positions and paths of floats, and a variety of other details which, taken together, would make at least a quarto volume containing many hundreds of pages. And to what purpose

would this expense have been incurred, when we find that certain important details which have been given in the fullest manner in the Report, are asserted by Dr. Hagen not to have been given at all.

In truth, if Dr. Hagen had attempted to impartially weigh, instead of to attack, our conclusions, he would have seen that our theory suggests why it may be expected that a combination of many observations should closely represent the normal form of the curve. The discrepancies usually exhibited by single measurements are largely due to oscillations of the horizontal axis of the parabola, which repetition soon eliminates. It is a fact, now well known, that Boileau, Bazin, Grebenau, Ellis, and other observers have obtained mean curves very closely agreeing with our parabolic law. Indeed, Grebenau found that still older observations accorded with it so well that he wrote: "It is remarkable that the law of decrease which is obvious in all these earlier experiments could have remained so long unknown. If, however, it be remembered that Brünings only worked with logarithms, and made no attempt to represent former measurements, or his own observations, graphically with compass and scale, it will be understood how the discovery of the law was so difficult."

We cannot better close our remarks upon Dr. Hagen's attack upon this part of our work, than by quoting from a paper published in 1875 by M. Bazin, a distinguished Engineer of the Ponts et Chaussées, whose labors and writings place him confessedly in the foremost rank of living hydraulic engineers. He writes:

"The distribution of velocity in flowing water has been made the subject of numerous experiments, which are far from being accordant even for the simplest case—that of a canal of indefinite width, where the effect of the sides can be neglected. When a large river is dealt with, these experiments present considerable practical difficulties, and the eminently variable and capricious nature of the phenomena, in which many secondary influences mask the true laws, add to these difficulties. Nevertheless, hydraulicians now generally admit that the velocity upon a single vertical, varies as the ordinates of a parabola. The maximum velocity is sometimes at the surface, and sometimes below, although no one has as yet succeeded in giving a satisfactory explanation of the causes which induce its changes in position. According to this parabolic law the velocity at any given point upon a vertical, may be deduced from its depth d below the surface, by the very simple formula:

$$v = V - m\left(\frac{d - d_1}{D}\right)^2"$$

This is our formula. It was first announced in the Physics and Hydraulics of the Mississippi; and against it was directed the attack to which we have just replied, and which was published ten years ago. If Dr. Hagen and his admirers had not recently quoted this old attack as proving that our data were suspicious, and "probably altered in part to establish the theory proposed," this reply would not have been deemed necessary. At its date, and indeed until very recently, it failed to come to our notice.

Taking leave of the subject of the change of velocity below the surface, we now proceed to consider very briefly Dr. Hagen's strictures upon our formula for the mean velocity of a flowing river, as he has presented them in his Investigations on the Uniform Motion of Water, published in 1876.

He admits the great value of our 19 observations for discharge, slope, etc.; asserts that the true method of deriving a formula from them is the method of least squares; assumes the general expression for the velocity in terms of the slope and mean radius to be $v = As^x r^y$; and decides that the value of his constants from our observations should be:

$$A = 7.645 \quad x = 0.2271 \quad y = 0.51.$$

Applying this formula to the observations from which it was deduced, he finds the sum of the squares of the residuals to be 0.6858. He then remarks:

"Humphreys derived another analytical expression from his observations which we ought not to omit giving to the reader, since immediately after the publication of the translation of the American work, the attention of the German engineers was directed to the superiority of this new theory over all the older ones."

He proceeds to misquote our formula in so gross a manner as to show that the proofs of his paper were corrected with culpable negligence. But even this treatment is better than we received in his former article, in which he quoted instead of our formula an approximate expression the application of which we had carefully restricted, and applied to it criticism that derived its whole weight from this substitution. He continues:

"By this most inconvenient formula Humphreys himself computed the velocities; and the differences between these results and the observations are given in the last column of a table on page 317 of his work. The sum of the squares for these 19 observations amounts to 1.5296. This is 2½ times as large as that given above, while the probable error amounts to 0.21 feet; hence it seems superfluous to recur to this theory again."

In reply to these views of Dr. Hagen we will say, that, in our opinion, he has adopted an arbitrary and mechanical method of discussing the observations, which is open to criticism. The object proposed in making these measurements was to discover from them the natural laws which govern flowing water, and to deduce a formula which would truly represent these laws, and not one which would give the smallest probable error when applied to the limited data available. To do this, it is not admissible to arbitrarily assume the form of the equation. This must embody all the known laws affecting the variables. The observations, when few in number, should determine the numerical values of the constants, *not so as to make the sum of the squares of the residual errors a minimum, but so as to fulfil the most probable conditions suggested by careful mental study.* In such an investigation, the graphic method possesses incontestable advantages over that of least squares; and we therefore gave it the preference.

Whether Dr. Hagen or ourselves be right in these opposite views as to the proper manner of treating the problem mathematically, admits of a direct test.

It will not be denied that the best proof of merit in a formula of this nature, is the correct prediction of results afforded by new measurements not available in deducing its constants. Our formula was based upon 30 standard measurements; of which 19 were our own, and 11 had been published in such detail as to warrant a belief in their accuracy. There are now available in addition, 49 similar observations published by Darcy and Bazin; 15 published by Grebenau; and 4 made upon the upper Mississippi by General Warren and Mr. Clarke. The whole will be found in Johnson's Cyclopædia (article Rivers, Hydraulics of). Out of these 68 new observations which were not available when our formula was framed, no less than 42 largely exceed the limits in respect to cross-section and slope within which we restricted its use. The test of its general applicability to natural channels which they afford, is therefore exceedingly severe. Nevertheless, the mean discrepancy for the 98 observations is only 9 per cent,—much less than for any other single formula which has ever been proposed, and 1 per cent. less than Dr. Hagen thinks it reasonable to expect from such a formula.

All these observations, except probably the last four, were available and known to Dr. Hagen when deducing his latest formula of 1876. In this work he proceeded upon his general method indicated above; abandoned the attempt to frame a single formula; and finally adopted two radically different expressions, one applicable when the mean radius is less than 1.5 English feet, and the other when it is greater than this quantity. These expressions in English feet are respectively:

when $r<1.5$ $V=4.9\ r\ \sqrt[5]{s}$

$r>1.5$ $V=6\sqrt{r}\ \sqrt[4]{s}$

When this double formula is applied to the 98 standard observations, the mean discrepancy is 12 per cent.

In fine, then, from 30 observations in 1860 we were able by our method to frame a general river formula which gives a mean discrepancy for these 98 standard observations of only 9 per cent; while Dr. Hagen in 1876, by his method, is only able to reduce his discrepancies to 12 per cent.—and that, by resorting to the expedient of using a double formula. Comment seems to be superfluous, except perhaps to suggest that Dr. Hagen's polite assumption that the familiar method of least squares "was probably unknown" to us is not necessary

to account for our preferring our own method of analysis.

In conclusion, we may say that the investigations of the Mississippi Survey were conducted with the sole desire to develop truth. The contributions to the science of hydraulics were not the end sought; but rather the means by which practical conclusions involving immense financial interests might safely be reached. The first use of the discoveries, long before they were published to the world, was made in discussing the problem of protecting the alluvial region of the Mississippi against overflow. As our professional reputations were at stake in arriving at correct conclusions in this matter, which, sooner or later, will surely be put to practical proof, we gave every step of our analysis a scrutiny more severe and thorough than it probably will again receive. At any rate, the critic may rest assured, at the outset, that we committed no errors so gross and absurd as those which Dr. Hagen has imagined.

THE "GEOMETRY OF POSITION" APPLIED TO SURVEYING.

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I.

INTRODUCTORY.

I. I PURPOSE to set forth as briefly as possible, a few results obtained from the application of the "*Geometry of Position*" to the solution of such problems in surveying as are of every-day occurrence, believing that the results thus obtained will not prove altogether uninteresting or unprofitable. As the *Geometry of Position*, however, is a branch of mathematics, scarcely known even by name in this country, some statement of its peculiar character and chief merit seems quite in place by way of preface.

That such a statement can be justified on such grounds, is, to say the least, a matter both of regret and surprise. Of regret, that so simple, so beautiful, so eminently useful a branch of mathematics has been suffered to remain so long unheeded. Of surprise, that in spite of the relation in which Geometry stands to all other branches of mathematics, a most important advance in Geometry is quite unknown; that in spite of the efforts so persistently made to simplify and reduce all mathematical processes, a most effective agent for this purpose is yet unused. Nothing perhaps is more characteristic of the present state of the mathematical sciences than the simplicity of their solutions. It seems, indeed, as if that inventive spirit which in the industrial arts has led to the production of numberless labor-saving machines, has invaded even mathematics, and led to the production of all manner of labor-saving processes of solving long and difficult problems, till now it is possible to solve graphically, with the ruler and pencil, any problem from a proposition in the rule of three, to the determination of strains in the parts of the most complicated engineering structure. In the industrial arts such labor-saving machines are said to be the direct result of a lack of skilled manual laborers: perhaps in mathematics such labor-saving processes may, to some extent, be the direct result of a lack of skilled intellectual laborers. However this may be, the Geometry of Position, though capable of accomplishing much in the above mentioned respect, has as yet been turned to no use. Indeed it is taught *nowhere* out of the Polytechnic schools of Germany, while a text-book on the subject in English remains to be written.

As a branch of Geometry it is peculiar in all respects. If the story that has come down to us from the time of Thales is deserving of any credit, the science of Geometry arose from the efforts of the Egyptian priests to restore the landmarks and boundaries washed away by the yearly inundations of the Nile, and

to determine the areas covered by the fertilizing waters; a statement which the Greek name Geometry—"*Land-measuring*" tends not a little to confirm. But whatever may have been its origin this much is certain, that Geometry as a science first appeared in the valley of the Nile, and that among the Egyptians it held very much the same place surveying holds with the nations of to-day. At the very beginning, therefore, Geometry became associated with the idea of measure, and so firmly has this been clung to, that ever since, in whatever form the science has appeared, whether as Trigonometry or Analytical Geometry, magnitude or quantity has formed the basis. It is now universally conceded that measure is not an essential element of Geometry; and that while there is a *Geometry of Magnitude* or *Quantity* there is also a *Geometry of Figure.* Almost within our own day the science has been divided into the "*Old Geometry*" or "*Geometry of Measure*" and "*Modern Geometry*" or "*Geometry of Figure*" under which is to be placed the "*Geometry of Position.*"

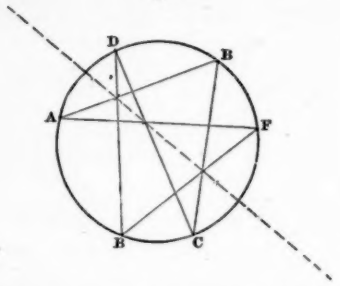
II. THE GEOMETRY OF POSITION

differs essentially from the Old Geometry or Geometry of Measure in three particulars; in the simplicity and paucity of its elements; in the total absence of the idea of measure and all metrical relations; and in the great generality and comprehensiveness of its principles and problems.

The fundamental elements of the ancient geometry are, the point, the line, the plane, the angle, the circle, solid bodies, surfaces of revolution and the long array of triangles, rectangles and polygons. The fundamental elements of the *Geometry of Position* are the point and line. The most marked peculiarity however, is the total absence of all metrical relations. In the old geometry the metrical relations of the parts of the figure are never for a moment lost sight of. It is the length of some line or the bisection of some angle, the equality or similarity of some figures, the value of the square of some side that is to be demonstrated. It is a proposition on the intersection of medial lines, on the measure of inscribed angles; on the ratio of homologous sides; on areas, on volumes, on circumferences, on perimeters. The Geometry of Position, on the other hand, takes no account of measure either angular or lineal. In none of its problems is there to be found any mention of perpendicular or oblique lines, of angles, of areas, or of volumes. It has nothing to do with triangles whether right or oblique, isosceles or scalene; nothing to do with rectangles or parallelograms, or with regular or irregular polygons. No line is ever bisected; no angle is ever read. The student therefore, who, familiar with the principles and problems of the Old Geometry, enters on the study of the Geometry of Position, finds himself, so to say, at the beginning of a new science. Theorems and axioms, corollaries and scholiums, which he has long looked on as the very frame work of geometry, are utterly abandoned, and, without considering the length of a single line or the measure of a single angle, he enters on the solution of problems of the utmost generality and comprehensiveness. In the Old Geometry of measure the majority of propositions are necessarily limited in the scope of their application, the conditions on which their demonstration depends, are particular rather than general. Change the length or inclination of a line, alter the measure of a single angle, and the proposition falls to the ground. Propositions that are true of right angled triangles are not true of oblique angled triangles. Propositions true of equilateral are not true of scalene triangles. In the *Geometry of Position* whatever is true of *one* figure of three sides is equally true of *every* figure of three sides, no matter how long or short the sides may be, or how various their inclination to each other. In the Geometry of Measure again, what is true of the circle is not necessarily true of the ellipse, the hyperbola, and the parabola, but such is the comprehensiveness of the Geometry of Position, that *every* proposition true for the circle is true for the ellipse, the parabola, the hyperbola, in short, for every curve of the second order. The length of the radius, the position of the foci, the length of axis is never for a moment considered. It is the *position* of lines, not measure, that determines all things. Thus it is one of the propositions of the geometry of position that if any six points be

taken *anywhere* in the circumference of a circle and joined by consecutive straight lines in *any order whatever*, then will the three points of intersection of the three pairs of opposite lines lie in one and the same right line; an illustration of this is given in figure 1. The points

Fig. 1.



are there taken in the order ABCDEF, and joined by the six consecutive lines, AB, BC, CD, DE, EF, FA; the three pairs of opposite lines intersecting in the three points *a*, *b*, *c*, which lie in the right line *ac*. By opposite sides is meant every *first* and *fourth* line taken consecutively. Thus beginning with the line AB, the *fourth* line in consecutive order is DE, therefore AB and DE are opposite lines; so are BC and EF; so are FA and DC. Now this proposition is true not only for *any* six points taken *anywhere* on the circumference of a *circle* and joined in *any order whatever* by consecutive lines, but for *any six points* on an *ellipse*, a *parabola*, an *hyperbola*, or in fine for *any* six points on *any* curve of the second order. The proposition is therefore general and comprehensive to the last degree. Yet it takes no account whatever of measure or metrical relations.

Two other propositions may perhaps afford a yet clearer idea of the character of the propositions of the *Geometry of Position*. It is, however, but just to state that they have not been *especially* selected for this purpose, but are here introduced mainly because they are to be applied later to the solution of problems in surveying. The first of these is to this effect. If A, B, C, be *any* three points in *any* three concurrent lines, and A′, B′, C′, any three other points in the same lines, and *a*, *b*, *c*, the three intersections of the three pairs of connectors BC and B′C′; CA and C′A′; AB and A′B′; then are the points *a*, *b*, *c*, in the same right line. The proposition is extremely general, the points may be taken anywhere in any three concurrent lines, and however great or small the angles which the lines make with each other, the proposition is invariably true. It is apparent from the figure that the statement of the proposition may be varied as follows: If any two right lined figures of three sides each, as ABC and A′B′C′ (Fig. 2), be so placed that the connectors of each pair of corresponding points A and A′; B and B′; C and C′ meet in a point P; then the three points *a*, *b*, and *c* of intersection of the three pairs of corresponding sides BC and B′C′; AC and A′C′; BA and B′A′ are three points in a right line. No account is taken of the distance these figures are apart; no account is taken of the length of the sides, nor of the angles they make with each other. Yet the proposition is invariably true for all sizes and shapes of figures having three right-line sides when so *placed* that the lines joining each pair of corresponding points pass through a common point.

The second proposition is this: If A, B, C and D be any four points of intersection of *any* two pairs of concurrent lines and the points of concurrence *a* and *c*, of these lines be joined by the line *ac*, then will the connectors AC and BD of the two pairs of opposite points A and C, B and D, meet the line *ac* in two points *b* and *d* such that if either one with the two points *a* and *c* remain fixed, the other will also remain fixed for all possible positions of the points A, B, C and D. Or, to state it differently, if ABCD be any right lined figure of four sides, and the two pairs of opposite sides AB and CD, BC and AD be produced till they meet, then will the two diagonals AC and BD when produced cut the line *ac* in two points *b* and *d*, such that if either one with the two *a* and *c* remain fixed, the other will also remain fixed for all possible positions of the figure ABCD. This is illustrated by the figures on the right and left of ABCD. Here again no account is taken of measure. The length of the sides, the distance of the figure from the line *ab*, the measure of the angles has nothing to do

with the proposition. So long as the *position* of the sides is such, that each pair of opposite sides intersect when produced, the proposition is *invariably true.*

III. ANHARMONIC RATIO.

This singular result should seem to indicate the existence of some fixed relation between the four points *a*, *b*, *c*, *d*, a relation which on examination is found to be of the very highest value in the application of the Geometry of Position to practical uses. The four points *a*, *b*, *c* and *d*, will, it is evident, always lie in a right line, and possess relations and properties that may be briefly summed up as follows: The four points are four anharmonic points, constitute an anharmonic range, cut the line anharmonically, have an anharmonic ratio, and taken in pairs are the anharmonic conjugates of each other, that is to say *a* and *c* are anharmonic conjugates, as are also *b* and *d*.

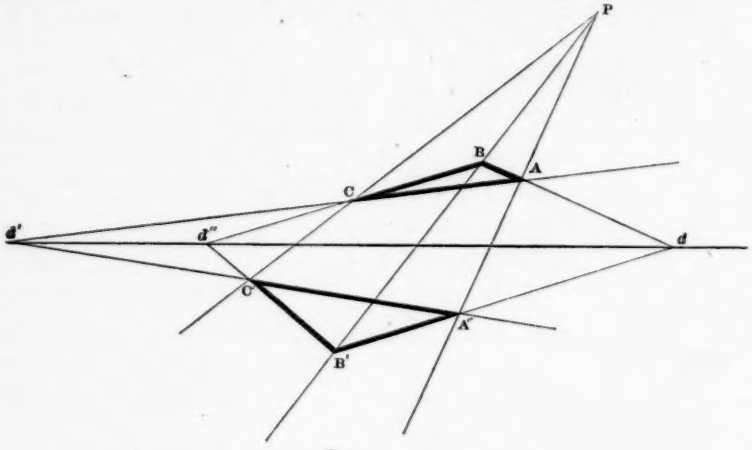


Fig. 2.

By anharmonic ratio is meant simply this. If any line as *ac*, Fig. 3, be cut at any two points *b* and *d*, the line is cut into six non-adjacent segments, or segments having no end in common. These are *ac* and *bd*; *ab* and *cd*; *ad* and *bc*. Now, if any two pairs of non-adjacent segments be taken the ratio of ratios of these two pairs is the *anharmonic* ratio of the section of the line. Thus, if the sections taken be *ac* and *bd*; *bc* and *ad*, then the ratio of ratios is

$$\frac{\frac{ca}{cb}}{\frac{da}{db}} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Or again to express it more simply, if *a*, *b*, *c* and *d* be any four points on a right line, and these points be taken in any order whatever, the ratio of the first and second to the third, divided by the ratio of the first and second to the fourth is the anharmonic ratio of the four points. So that the four points being taken in the order *a*, *b*, *c*, *d*, the anharmonic ratio is by the above definition

$$\frac{\frac{ac}{cb}}{\frac{ad}{bd}} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

an equation precisely similar to equation (1). This ratio for the sake of brevity is written [*abcd*], the order of the letters indicating the order in which the points are taken.

As these points may be taken in *any* order, and as the four letters representing them may be written in twenty-four different combinations, it should seem that there should be twenty-four different anharmonic ratios for each range of four points. When, however, the anharmonic ratio for each of these twenty-four combinations is written out, it ap-

pears that there are but six anharmonic ratios differing in value, and that each one of these six may be written in four different ways *without altering its value.* Thus, taking the points in the order *a, b, c, d,* we have

$$[abcd]=\frac{ca}{cb}:\frac{da}{db}=\frac{ca\,.\,db}{cb\,.\,da}$$

$$[badc]=\frac{bd}{ad}:\frac{bc}{ac}=\frac{bd\,.\,ac}{ad\,.\,bc}$$

$$[cdab]=\frac{ca}{da}:\frac{cb}{db}=\frac{ca\,.\,db}{da\,.\,cb}$$

$$[dcba]=\frac{db}{cb}:\frac{da}{ca}=\frac{db\,.\,ca}{cb\,.\,da}$$

The anharmonic ratio $\frac{ca}{cb}:\frac{da}{db}$ may therefore be written in either of the four ways $[abcd]$, $[badc]$, $[cdab]$ or $[dcba]$. So may each of the five other ratios be written in four ways, without a change of value; the six different ratios and the four different ways of writing each, accounting for the twenty-four combinations of the points *a, b, c, d.*

When the value of the anharmonic ratio is unity, the points are said to be harmonic points and the ratio an *harmonic ratio.*

In connection with this matter of anharmonic and harmonic ratio it is to be remarked, that the truth of this statement that the Geometry of Position takes no account of metrical relations, is not at all impugned by the fact that these *ratios* are based on the measure of the segments. They are, so to say, afterthoughts. The propositions of the Geometry of Position are wholly independent of anharmonic and harmonic ratios, which are, indeed, results derived from the Geometry of Position. In many instances, however, where a practical application of this Geometry is made, the necessity of obtaining the measure of lines arises, and then is it that these ratios are found invaluable. Such is the case when applied to the solution of problems in surveying, and on this account it has been judged best to briefly introduce the matter of anharmonic ratio before taking up the matter of surveying by the Geometry of Position.

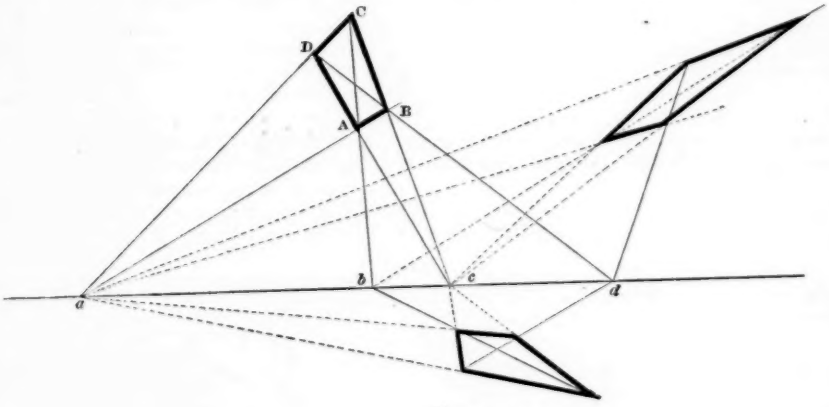


Fig. 3

IV. SURVEYING BY THE GEOMETRY OF POSITION.

We shall begin with the proposition illustrated in Fig. 3. The problems in Engineering Field-work, to the solution of which this proposition is applicable, fall under one of three classes.

1° To prolong a line through or across some obstruction, as a house, a marsh or pond.

2° To obtain the measure of an inaccessible or immeasurable distance.

3° To locate and find the distance to a remote point.

1° TO PROLONG A LINE THROUGH AN INVISIBLE POINT ON THE LINE.

The problems in this class are undoubtedly the most frequently met with

of all, and the methods of solving them now in use, by perpendiculars, by equilateral triangles, by similar triangles, and by triangulation are so familiar that it is unnecessary to do more than recall the fact that they all depend on angular and chain measurement, and that the character of the ground surrounding the obstacle to be passed will in general determine which of the three is to be used.

The Geometry of Position, on the other hand, affords two methods of passing round an obstruction, each of which is based on the proposition regarding the right line figure of four sides; is wholly independent of angular and chain measurement, and is applicable in *all* cases, whatever may be the nature of the obstruction or the physical character of the ground. Suppose, for illustration, that in locating a tangent on a railroad, represented by the line AX in Fig. 4, some obstruction as a house is met with at O. Now, by simply reversing the third proposition, which is illustrated in Fig. 3, it becomes applicable to the present case. For it is evident that if from any two points a and b, on a right line, two pairs of lines be drawn in such wise as to form by their intersection a figure ABCD, the two diagonals AC and DB will cut the line ab in two points c and d. To apply this to the solution of the problem in hand it is merely necessary so to arrange that the three points a, c and b shall be on the line to be staked out and all three on the *same* side of the obstruction while the point d is on the other side of the obstacle. The first condition is easily satisfied since the points a, b, c may be taken *anywhere* on the line staked out before reaching the obstruction. All turns, therefore, on the determination of the point d at which the diagonal DB cuts ab produced, and this point may be found by either of two methods.

FIRST METHOD.

It will be remembered in connection with the proposition that if any three of the points remain fixed, the fourth point will also remain fixed for *all* positions of the figure ABCD as illustrated in Fig. 3. That is to say, if a, b, c are fixed points, the diagonal DB will *always*, for all positions of ABCD, pass through the same fixed point d. In order to find d on the ground it is necessary merely to lay out two figures ABCD and A′B′C′D′, and find the intersection of the two diagonals CD and C′D′. This will be a point in ab produced. The simplest method of doing this is illustrated in Fig. 4, and

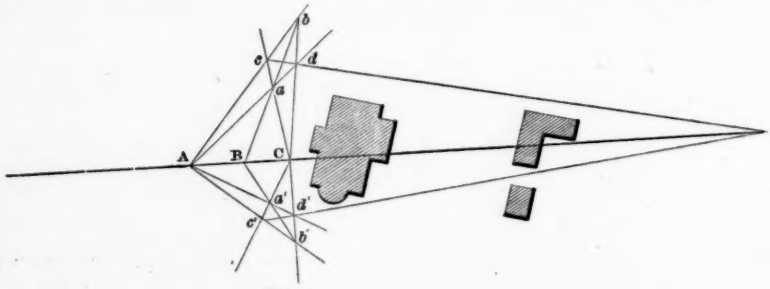


Fig. 4

is performed as follows. Select *any* three points as A,B and C in the line previously located; set the instrument at B, and facing the obstacle turn the telescope off to the right of the line and locate two points, a, and b, in the line of sight such that from each of them a sight can be had past the obstruction. Now turn the telescope to the *left* of the line and in the same way locate a' and b' in the line of sight. Move to A and sight to b and locate a part of Ab near where it seems to cross a line joining aC. Then sight to a and locate a part of Aa near where it seems to cross a line joining bC. Next sight to b' and a' repeating the operation just described. This has all been done without moving from A. Now set the instrument at C, sight to a and locate c; also sight to b and locate

d; then to *a'* and locate *c'* and to *b'* and locate *d'*. Finally, run out *cd* locating a small portion of it about where it seems likely to cross *ab* produced, and the point in which *c'd'*, when run out, intersects *cd*, is the point D in the line *ab*.

In passing round an obstruction by this method it is evident that no angles are read and no measurement of lines made. Nor is it necessary; for the angles which the lines from A, B and C, in the drawing, make with the line AB, has nothing whatever to do with the proposition, and hence the angles through which the instrument is turned from the line of sight is a matter not to be considered in the application of the proposition. Neither is the matter of the distance of the points *a* and *b*; *a'* and *b'* to be regarded, since the length of the lines in the figure has nothing to do with the solution. We are, therefore, in all respects at liberty to choose our ground and to turn the telescope to the right and left of the line to be prolonged, without regard to the angle turned through. Two things, however, are to be avoided. If the points A and C are so chosen that AB=BC, then the lines *cd* and *c'd'* will be parallel to each other and to the line to be prolonged,—in other words will not *intersect* AB. But inasmuch as these points A, B, and C are taken at pleasure and without measurement, it is quite improbable, indeed almost *impossible*, that AB will ever be made equal to BC, and hence no trouble need be borrowed on this account. Yet this serves to illustrate a feature of this system which will very often be found of use. When AB is exactly equal to BC, the lines *cd* and *c'd'* meet nowhere; consequently, when AB is a *little* greater than BC the lines *cd* and *c'd'* will *meet*, but at a *great distance*. For instance, as proven in equation A', if AC be one hundred feet long and AB=50.003 feet, BC will equal 49.997 feet and CD will be over *one hundred and fifty seven miles* long. Had CB been 49 feet then CD would have been 2450 feet. To apply this: if at any time it is necessary to prolong the line through a *series of obstacles*, as illustrated in the Figure 4, the points A and C have only to be taken so that BA, judging by the eye is a few feet greater than BC, and the lines *cd* and *c'd'* will meet beyond the obstacles. Finally, that the lines *cd* and *c'd'* shall *invariably* meet *beyond* the obstruction, the lines of sight from B, to *b* and *b'* must point towards the obstacle. The consequence of pointing these backwards is shown in Fig. 5. The diagonals *cd* and *c'd'* will, even in this case, meet AB, but at a point behind A as at D'.

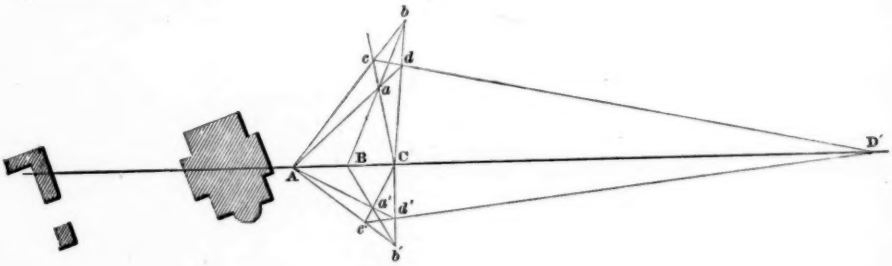


Fig. 5

THE SECOND METHOD.

The second method of determining the point D, is much shorter than that just given and not less accurate. Take *any point* as *a* (Fig. 6), not in the line to be prolonged, from which it is possible to see past the obstruction on each side. Set the instrument at this point, and turning the telescope in the direction of D, (so as to clear the house or whatever the obstacle may be) locate *any* point in the line of sight, as *c*, and also locate on the ground a small portion of the line near where, in all probability, it will cut AC produced. Then (from the same point *a*) sight to *any* point in the line AC, to be produced, and near the obstruction at C, staking out a small

part of aC as ad. Next sight (from a) to *any other* point in the line AC further from the obstacle than C, locating a portion of this line aA as ab. The location of the lines ad and ab may be speedily effected in this wise. The points C and A, are taken *anywhere* in AC, as is also the point c in aD. To locate ad therefore, drive a few stakes

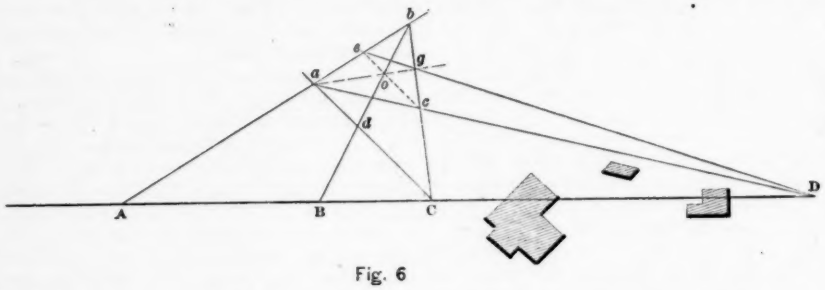


Fig. 6

find g and b exactly; then sight to A and find d exactly. Move to d, sight to b and find o. Return to c, sight to o and find e. Finally move to e, sight to g, and find the intersection of the line eg with the line ca. This will be the point D, a point in the line AC produced through the obstruction.

If these two methods of prolonging a line through an obstruction, as illustrated in Figures 4 and 6, appear quite complicated, it should be remembered that the lines of the figure have no existence on the ground, while a few applications to practice will show the utility of the system, and the ease and rapidity with which the work may be done. They are superior to the old method, in that they are wholly independent of both angular and chain measurements, and thus leaving the transit man free to choose his ground are applicable in any case and on all kinds of ground. As to their accuracy, some idea may be formed by performing on paper with a ruler and pencil the operations described in the text. Repeated trials have demonstrated their accuracy in the field.

These and indeed any other methods of passing around obstructions, would not be of much practical value did they fail to afford a way of determining the break in the line prolonged; in other words CD, (Fig. 6) a problem the Geometry of Position solves in a singularly beautiful way. Under the head of anharmonic ratio it has been stated that the four points a, b, c and d of Figure 3 are four anharmonic points, and taken in any order whatever afford an anharmonic ratio. Taking them therefore in the order A, C, B, D, (Fig. 4) the anharmonic ratio is

$$AB : CB : : AD : CD.*$$

solving this we have

$$\frac{AB}{BC}=\frac{AD}{CD}=\frac{AC+CD}{AD-AC}$$

$$\therefore AB\times AD-AB\times AC=BC\times AC+BC\times CD.$$

But $AD=AC+CD$

$$\therefore AB\times AC+AB\times CD-AB\times AC=BC\times AC+BC\times CD.$$

reducing $CD(AB-BC)=BC\times AC$

$$CD=\frac{BC\times AC}{AB-BC} \quad . \quad . \quad (A)$$

To obtain the value of CD, it is necessary to know the values of AB and BC.

By assuming different values for these segments and substituting in equation A, two singular results follow. Suppose AB=40 and BC=20 feet; then

$$CD=\frac{20'\times 60'}{40'-20'}=60'$$

That is, if the distance BC be equal to

* This is evidently the harmonic ratio of the points when taken in the order ABCD.

one half the distance AB, then CD is equal to AC, a fact worth remembering when the first method of prolonging the line is used, as in that case the points A, B and C, are taken at pleasure, and can therefore be so taken that $BC=\frac{1}{2}AB$. Again if B is taken midway between A and C, so that AB and BC have each the same value, say 30 feet, then

$$CD=\frac{30'\times 60'}{30'-30'}=\infty$$

or the point D is at an infinite distance from C and the lines *cd* and *c'd'* (Fig. 4) are parallel to each other and to the line AC; a demonstration of the statement made regarding the first method of prolonging a line, namely, that in selecting the points A,B and C on the line AC, they must be so taken that B *shall* not be midway between A and C.

Finally, if AB be less than the segment BC, as, for example, AB=10′, and BC=15′, then

$$CD=\frac{15'\times 25'}{-5}=-75'$$

or the point D will fall 75′ to the left of C, or on the *same side of the obstruction* as the points A,B and C, as illustrated in Fig. 5. In practice the second and third results are never likely to occur. For, if the line be prolonged by the second method, neither *can* occur, since as *ac* is *so taken* as to pass the obstruction, the point B the conjugate of D *cannot possibly* fall so that AB shall be equal to or less than BC. When the first method is used the points A,B and C are taken at pleasure and may, therefore, be so taken as to avoid all trouble.

The accuracy of Eq. A, may be tested graphically, by assuming different values for AB and BC, laying them off to a scale on a right line, drawing from A *any* two lines as A*b* and A*c* of indefinite length, from B *any one* line cutting those from A in any two points as *d* and *b*; from C two lines through *d* and *b* cutting the two from A at *a* and *c* respectively, and drawing *ac* till it meets AC produced in D. Then, measuring CD by the scale used to lay off AB and BC, the value obtained for CD should agree with that found by substituting the values assumed for AB and BC in Equation A.

THE ARCHITECTURE OF LIGHTHOUSES.

From "The Builder."

LIGHTHOUSE architecture has very generally been entrusted of late to the engineer rather than to the architect. No doubt, however, can be thrown upon the statement that the problem to be solved is for the most part purely architectural. The first question raised is necessarily that of site. The reasons for erecting a lighthouse in any particular spot are indicated by Nature herself. A shore-light may have two distinct meanings. It may be a beacon, to warn from danger, or a sign of invitation to enter a place of safety. Lighthouses of the first order, however, are rather to be regarded as topographical land-marks. They are usually placed on capes or headlands, and indicate, for a distance of from ten to thirty marine leagues, the general outline and configuration of a coast. From this first order of distinct lights the importance of the signals in question descends to that of lights of the second, third, fourth, and fifth orders,—the last being signals placed on shoals or sand-banks, or on piers at the entrance of a port for the guidance of mariners at the very moment of coming into harbor.

The selection of site is always the first care of the architect. In regarding the site of a lighthouse his skill must be aided by the experience of the sailor and of the marine surveyor. Attention must next be turned to that of the preparation of the foundation. For distant lights this is usually a simple matter. In some cases, on the contrary, the utmost skill of both the architect and the engineer is taxed in order to provide an unassailable security for the base of a tower. This is especially the case when the light is intended to serve for a beacon or warning

to ships near at hand, as well as to form a distant light to indicate the bearing of a coast. Such was the case with that dangerous rocky shoal which lies in the chops of the Channel, about fourteen miles to the south-west of Plymouth Roads. A small point of rock alone appeared above the water; the rest of the shoal, stretching some 180 yards from north to south, is always covered by the sea. This rock is the first obstacle encountered by the heavy rolling waves of the Atlantic, and lies in the most direct course for an inward bound vessel.

From this visible point, fretted by the waves, the rock descends almost perpendicularly to a depth of from 30 to 65 fathoms. The result of this opposition of a vertical submarine wall is to throw the incoming waves, when urged by any considerable wind, with a prodigious leap into the air. A height of from 150 feet to 200 feet is said to be at times attained. It was the construction on this unusually dangerous and exposed situation of the famous Eddystone Lighthouse, by Smeaton, the engineer, which seems to have secured to his profession, thus far, something of a monoply of the construction of lighthouses.

In old times, however, the *Phari*, or light-towers, were among the most famous of the works of the architect. The name of Apollodorus, the famous architect of the Emperor Trajan, is yet associated with the port, breakwater, and Pharos of Civita Vecchia. The word *Pharos*, by which name so many lighthouses are now called, especially by the French, is of Egyptian origin. It was derived from the Island of Pharos, on which the famous lighttower of Alexandria was built. First given to the tower in question, it was thence imposed on similar structures. Such, at least, is held to be the force of the line of Statius,—

"Lumina noctivagæ tollit Pharus æmula lunæ."

The classical scholar will observe that the quantity of the word *Pharos*, as determined by this line, disposes of the derivation which has been sometime proposed from the Greek verb, meaning to shine.

This famous tower was of a quadrangular form. It rose in the midst of an *enceinte* of 220 yards on each side, containing accommodation for a garrison. It was situated at the eastern angle of the new port of Alexandria, in a line with the point called Pharaglione. It was commenced under the reign of Ptolemy Soter (B.C. 324 to 285), and completed under his successor, Ptolemy Philadelphus B.C. 285 to 247. Its architect was Sostratus of Cnidus; and it bore an inscription dedicatory to the gods protectors of mariners. The legend is to the effect that the name of Ptolemy alone appeared over the gate of the tower when first completed. This commemoration, however, was made in plaster; on the subsequent decay of which the name of Sostratus appeared, graven in marble. The height of the tower is stated at a little more than 150 feet. The structure has been represented as a Doric base, with columns and pilasters, surmounted by seven blocks of masonry, decreasing in size, each finished with a projecting cornice. On the top of the whole an immense fire was kept burning by night, which was visible at sea twenty marine leagues off. The interior was chambered, and the position of the staircase was indicated by that of the windows. The Arab writer, Abulfeda, speaks of this Pharos as existing in his time, the thirteenth century. It has been described also by Edrisi, who lived at the commencement of the twelfth century. It ranked among the seven wonders of the world. Few vestiges can now be traced of this famous building. The modern Pharos of Alexandria is a new tower erected at another point.

While the Pharos of Alexandria is the most famous lighthouse of antiquity, it is not the most ancient. On the promontory of Sigæum, in the Troad, a lighthouse is said to have existed 650 years B.C., which served as a landmark by day, as well as for a beacon at night. And three centuries or more earlier than this, the vessels of the Greeks, returning from the siege of Troy, are said to have been lured on to the shoals near the island of Negroponte by fires lighted by the King of Eubœa with that design.

Another of the famous seven wonders of the world was the Pharos of Rhodes. This was a bronze statue, of upwards of 100 feet in height, according to Pliny, dedicated to Apollo. It stood astride on two blocks of masonry, which formed

the entrance to the port of Rhodes; and held aloft a sort of censer in its right hand, in which a beacon fire was kept alight by night. This colossus was erected by the citizens of Rhodes, in memory of the siege sustained against Demetrius, king of Macedon. It is said to have been actually constructed of masonry, covered with metal plates. It was overthrown by a violent earthquake in the year 224 A.D.

A lofty and very ancient tower looks down upon the Atlantic Ocean from the entrance of the port of Corunna, in Spain. It is called the Pillar of Hercules; and it is thought that the name Corunna is a corruption of the word "columna." By some writers the origin of this tower is attributed to the Carthaginians; by others to Caius Servius Lupus, who dedicated it to Mars. It was restored by Julius Cæsar; and again restored by Trajan. Its architectural form betrays an origin of a very remote date. There is a tradition that it was erected by an early king of Spain, in heoric times.

At Ravenna a large square tower which stands out from the side walls of the Church of Santa Maria in Porta Fuori is now used as a campanile or bell-tower. It is said to have formerly been the Pharos of the port which Augustus constructed at this point. In the fifth century the silting up of the Adriatic littoral (which is now threatening the destruction of the lagoon of Chioggia) had so obliterated the port of Augustus that its site was converted into gardens.

The Pharos of Boulogne is one of the most ancient in France. It was erected by Caligula (A.D. 38–41), on his expedition into Gaul, according to Suetonius. For many ages this tower was known by the name of the *Turris ardens*. Its base was octagonal, each side measuring eight métres, according to the design given by Montfaucon. It consisted of twelve stories, each smaller than the one below it, and each surmounted with a cornice. In the year 807, Charlemagne ascended this tower in order to review his army, and ordered that the long-extinguished beacon should be re-kindled. Various colored stones, brown, red, and yellow, were employed in constructing the different parts of this Pharos, with the aim of rendering it a more strikingly visible object. In 1545 the English made use of the tower as a fortress. Their works seem to have interfered with the solidity of the foundation, for in 1644 the tower fell.

Under Claudius, whose tenure of empire lasted from A.D. 41 to A.D. 54, a tower was erected, according Suetonius and Dion Cassius, at Ostia, in imitation of the Pharos of Alexandra. In the fifteenth century it was visited and described by Piccolomini, who was afterwards pope, under the name of Pius II., from 1458 to 1464. From a bas-relief in the possession of Price Torlonia, it is deduced that this tower was built, as that at Boulogne was previously described to have been, in successively diminishing planes or stories, with a fire kindled on the top. The elevation, however, which is produced by Signor Cialdi, in his learned memoir on lighthouses, from which very much of our information is derived, is that of three towers, of diminishing sizes, standing one on another, with a thick short column on the top supporting the beacon or bonfire. The deposits brought down by the Tiber have long filled up the Claudian port of Ostia; and the careful measurements which have been made by the Italian engineers of the position of the shore line at successive historic epochs, indicate that a geological undulation, similar to that witnessed by the famous temple of Serapis, at Pozzuoli, on the Bay of Naples, has occurred at the mouth of the Tiber.

The restoration of the lighthouse built by the Emperor Trajan at Civita Vecchia has been effected by the present Pope, Pius IX. The architect, Apollodorus, by Trajan's command, designed and carried out a commodious port at Civita Vecchia. A breakwater, or island, was placed before the entrance of the harbor, to protect it from the waves. Two towers were erected on this work, one of which sustained a beacon. In the Itinerarium, a journey through Etruria, of Numazianus, who lived in the fourth and fifth centuries of our era, the double towers and double entrance of the port are commemorated in hexameter verse. Neglect, and the action of the sea, gradually filled the port; and in the popedom of Gregory IV. (A.D. 828–844) it is spoken of as having been intentionally

ruined, for fear of being seized by the Saracens, and used by them as a naval centre. Under Urban VIII. it was repaired and re-opened. In 1616, Paul V., among other improvements, rebuilt the existing eastward tower. The westward towers were still in ruins at the commencement of the present century. It was commonly called Il Marzocco. Pope Gregory XVI. reared a small fort on its ruins, but no better signal was offered to navigators than a light hoisted on a mast. In 1860, Pius IX., consulted the illustrious astronomer, Padre A. Secchi, as to the lighthouse, and the present tower was built, and furnished with a Fresnel illuminating apparatus of the second order. The tower is of stone, clylindrical in form, and carries a lantern 110 feet above the level of the sea. The works were executed by the engineer Chevalier Giovanni Minti.

The Cordovan Pharos is a tower of admirable construction, reared on a rock at the mouth of the Gironde. The name is taken from that of an architect, who, about A.D. 830, built a tower in the island of Andros. Both town and island were engulphed by the sea in the earthquake of 1427. The present town stands on a shoal, seaward of the former Pharos, which is covered with three métres of water at high tides. It was commenced in 1545 by the Parisian architect, Louis D. Foix, by command of King Henry II. It was continued by Henry IV. in 1601, and finished under Louis XIV. in 1665. In 1778 the upper part of the tower was removed, and replaced by a frustrum of a cone, rising to 78 mètres from the foundations, by the architect Theulere. The tower is composed of a series of galleries rising one above another, ornamented with pilasters and friezes, and gradually diminishing in dimensions. From the second story a fine staircase conducts to the top of the cone. The latter is divided into four stages, on the last of which the lanthorn is placed. A wall surrounds the base, the diameter on the plan being 43 métres. Within this are situated the rooms of the guardians, almost in the form of casemates. It is claimed for this Pharos that it is the finest edifice of the kind in the world. In 1790 a reverberating light was placed on this tower. In 1854 a dioptric Fresnel apparatus was substituted, with a light of the first order, eclipsing from minute to minute.

The beautiful Torre del Capo at Genoa is well known to all familiar with that pictusesque Italian port. Originally built on the promontory of San Berrique in 1139; it was first illuminated in 1326. It was removed in 1512, and rebuilt, by the Republic, in 1643. It is a square tower, in two stories, with battlemented terraces, the lower portion nine méters square, the upper seven. Rising from a rock 42½ métres above the sea, it carries its light at the height of 118.5 meters above the water. In 1841 it was fitted with a Fresnel lenticular apparatus of the first order. For beauty and elegance of structure, this historic Pharos is one of the first in existence.

The Pharos of Meloria was built by the Pisans in 1154. It indicated the direction to be taken by ships making for Portopisano, and gave warning of a dangerous sand-bank. This tower was destroyed by Charles of Anjou in 1267, by the Genoese in 1287, and by the Guelfs in 1290. Having determined to abandon Meloria, the Pisans erected, in 1304, the lighthouse which still exists near Leghorn. It is celebrated by Petrarch. It stands near the entrance of the harbor, to the south of the new curvilinear mole; and rises 47 meters above the level of the sea. It is built of stone, in the form of two battlemented cylinders, surrounded at the base by a polygonal inclosure of thirteen sides.

On the shore extending from the extreme promontory of Istria as far as Chioggia, no lighthouse was in existence in 1818. In that year the flame was kindled on the Pharos of Salvore, which was built by the architect Nobili, on the Punta delle Mosche, twenty Italian miles from Trieste, and five from Pisano. Here occurred the first instance of the use of gas for lighthouse purposes. The tower is cylindrical, of wrought stone work, with a cornice above, and a quadrangular base. Its diameter is five meters, and the diameter of the cornice is 6.3 meters. An internal staircase leads up to the octagonal lantern, 4.4 meters high, and 3.8 meters in diameter, at the height of 35.5 meters above the level of the sea. The apartments for the attendants are in the square base of the tower.

The example followed more or less directly by the architect Nobili, as well as by George Stephenson in his plans for the Bell Rock lighthouse, built by him in 1808, at Inchcape, near Arbroath, was no doubt the great work of Smeaton before referred to, the Eddystone Lighthouse. The wonderful triumph attained at this most difficult and dangerous spot was without precedent among any of those ancient and famous Phari to which we have referred. The first attempt to raise a lighthouse to give warning of this destructive shoal was made by Winstanley in 1696. A base of stone, 15 feet in diameter, and 12 feet high, was built on the rock and fastened to it with iron cramps. On that was built the tower, 70 feet high to the vane. On the night of the 14th November, 1698, the light was first kindled. As the waves broke over the lantern at this height, an addition of 20 feet of masonry, and of 40 feet of metal work, was subsequently made, the wall of the tower being made a yard thicker at the same time. In November, 1703, it became evident that repair was necessary. Winstanley betook himself to the tower, expressing his confidence in its ability to withstand the utmost fury of the elements. On the night of the 26th of that month a fearful storm occurred, and the lighthouse had entirely disappeared by the morning. It is said that on the same night the model of the tower, which the builder had kept in his house in Littlebury, in Essex, fell down and broke into fragments.

In consideration of the great importance of the case to navigation, Parliament, in 1706, authorised the imposition of a light due on the vessels frequenting this part of the Channel, and a contract for a new lighthouse was made. A silk merchant, of the name of Rudyerd, undertook the erection. His plan was that of a plain wooden cone, lined with masonry, the idea being at the same time to provide weight for stability, and to oppose a smooth surface to the impinging waves. This tower whs 20 feet in diameter at the base, 14 feet at the top, and 62 feet high. The lantern was octagonal, 10 feet in external diameter, and 9 feet high. A globe of 32 inches diameter stood on the top of the lantern, at the height of about 90 feet above the level of the sea. After long and costly work this tower was completed in 1709. On the 2d of December, 1755, it disappeared in a few hours in a terrible conflagration, the origin of which is unknown. This unexpected destruction, owing to the fury of that element against which the builder had taken no thought to provide, furnishes a most instructive lesson as to the material necessary to employ for similar structures.

In 1756 Smeaton for the first time visited the Eddystone rock. The idea of the form to be given to the building he took from the trunk of an oak, as that provision which Nature herself had made for the most successful resistance to the play of the wind. A model made on this idea was approved by the Trinity House and by the Government, and Smeaton commenced the work in the August of 1757. The stone which he selected was the granite of Hingstone Downs, fifteen miles from Plymouth, for the exterior, Portland stone for the interior, and a mixture of Aberthaw lime and Roman pozzolana for cement. The stones at the base of the tower were bird's-mouthed and dove-tailed into each other and into the rock, so as to form an absolutely homogeneous structure. From August 27th to September 14th, 167 hours were employed in filling in the foundations. The utmost care was taken in the fitting and bonding of the stone, each stone being exactly worked, and fitted to those by which it was to be set, on shore, before sending to the tower. By August 8th, 1758, the work had been raised to the height of thirty-two feet, and the first store-room contrived in the tower was complete. On March 21st, 1759, Smeaton visited the spot, and found everything perfectly uninjured. From July 5th to August 7th, in that year, another thirty-four feet had been added to the height. The metal work of the lantern had been prepared under Smeaton's inspection in London, and the ball which formed the summit of the structure was fixed by his own hands at ninety-two feet above the level of the sea. The solid base rose for sixteen feet, at which height a door gave access to the internal spiral staircase.

On the night of the 16th of October, three years, ten months, and sixteen days after the conflagration of the work of

Rudyerd, the light was kindled in the lantern of Smeaton's Eddystone lighthouse. It has ever since given uninterrupted warning of the dangerous vicinity. Many violent storms have burst over it, especially that of January, 1762; but it has remained uninjured, although a distinct oscillation is felt in the lantern in violent winds. The calculation of the height of the waves that dash over this noble work makes them rise to the height of 162 feet above the level of the sea, giving a column of water containing from two thousand to three thousand metric tons. The necessity for the utmost exactitude in the joints of the masonry is thus apparent. When water exerts a battering force of this nature, its effect, if it gets under or within a building, is irresistible, while it breaks into foam against a smoothed and curved surface.

On the 24th October, 1877, Sir William Thomson, professor of natural history in the University of Glasgow, gave a lecture before the Shipmasters' Society on the subject of lighthouses; the point to which he chiefly directed attention, however, being not their architectural structure, but the character of their illumination. Ninety per cent. of our local lights are fixed; and the danger that they may be mistaken is said by the lecturer to be far greater than in the case of revolving or eclipsing lights. An eclipse, however, of from fifty to sixty seconds is far too long in dirty weather, and the eye may readily lose the point of light before its bearing has been correctly taken. The Craigmoor light, in the Firth of Clyde, has been fitted with a light of Sir William Thomson's own arrangement,—a long and a short eclipse alternate hours, and there is thus a short pause, giving sixteen seconds of unbroken light. This combination of flashes is made to signify the letter C, according to the Morse alphabet, a very beautiful application of one of the latest improvements in telegraphy.

A NEW GENERAL METHOD IN GRAPHICAL STATICS.

By HENRY T. EDDY, C. E., Ph. D., University of Cincinnati.

Written for VAN NOSTRAND'S MAGAZINE.

I.

ALL general processes used in the graphical computation of statical problems consist, in their last analysis, in a systematized application of the proposition known as the "parallelogram of forces," which states that if two forces be applied to a material point, and if they be represented in magnitude and direction by two determinate straight lines, then their resultant is represented in magnitude and direction by the diagonal of a parallelogram, two of whose sides are the just mentioned determinate lines. This is the basis of all grapho-statical construction, but the methods by which it is systematized, and the auxiliary ideas incorporated in the processes, have so enlarged its possibilities of usefulness, that Graphical Statics may perhaps claim to be a science of itself;—the science of the geometrical treatment of force.

In order to introduce to the public a new set of auxiliary ideas, which shall constitute a new method, of a character equally general with that now in use and known as the "equilibrium polygon method," it has seemed best to give, in the first place, a brief review of the principal ideas already employed by the cultivators of this science.

RECIPROCAL FIGURES.

When a framed structure, such as a roof or bridge truss, is subjected to the action of certain weights or forces, these applied forces form a system which is in equilibrium. Now any system of forces in equilibrium may be represented in magnitude and direction by the sides of a closed polygon, a fact which follows at once from the doctrine of the parallelogram of forces. Such a polygon is called the polygon of the applied forces.

Again, the forces which act at any joint of a frame are in equilibrium, and hence there is a closed polygon of the forces acting at each joint. The forces which meet at a joint of a frame are the longitudinal tensions or compressions of the pieces meeting at that joint together with any of the applied forces whose point of application may be the joint in question. Draw a diagram of the frame and the applied forces all of which we will suppose lie in a single plane. Call this the "frame diagram": it represents the position and direction of all the forces acting in and upon the frame. The frame diagram necessarily has at least three lines meeting at each joint. A piece which constitutes part of the frame does not necessarily have both its extremities attached at joints of the frame; one extremity may be firmly attached to any immovable object. The frame diagram is, therefore, not necessarily made up of closed figures.

Now draw the closed polygon of the forces applied to the frame, and at each of the joints where forces are applied draw the closed polygon of the forces which meet at that joint, using so far as possible the lines already drawn as sides of the new polygons, and at the same time draw polygons for the forces acting at each of the remaining joints. If this process be effected with care as to the order of procedure, as well as to the order in which the forces follow each other in the polygon of the applied forces, then the resulting "diagram of forces," which is formed of the combination of the polygon of the applied forces with the polygons for each joint, will contain in it a single line and no more parallel to each line of the frame diagram. In that case the force diagram is said to be a reciprocal figure to the frame diagram. If sufficient care is not exercised in the particulars mentioned, some of the lines in the force diagram will have to be repeated, and the figure drawn will not be the reciprocal of the frame diagram, nevertheless it will give a correct construction of the quantities sought.

If the frame diagram and the force diagram are both closed figures then they are mutually reciprocal. The properties of reciprocal figures were clearly set forth by Professor James Clerk Maxwell, in the Philosophical Magazine, vol. 27, 1864; in which is stated, what is also evident from considerations already adduced above, that mutually "reciprocal figures are mechanically reciprocal; that is, either may be taken as representing a system of points (*i.e.* joints) and the other as representing the magnitudes of the forces acting between them."

The subject has also been treated by Professor B. Cremona in a memoir entitled "Le figure reciproche nelle statica grafica." Milan, 1872.

We shall now give examples of this method of computing the forces acting between the joints of a frame, together with certain extensions by which we are enabled to treat moving loads, etc. The method is correctly called "Clerk Maxwell's Method." The notation employed, which is particularly suitable for the treatment of reciprocal diagrams, is due to R. H. Bow, C.E.; and is used by him in his work entitled "Economics of Construction." London, 1873. In this work will be found a very large number of frame and force diagrams drawn by this method.

Let the right hand part of Fig. 1 represent a roof truss having an inclination of 30° to the horizon, of which the lower chord is a polygon inscribed in an arc of 60° of a circle. If the lower extremities of the truss abut against immovable walls a change of temperature causes an horizontal force between these lower joints, the effect of which upon the different pieces of the truss is to be constructed. No other weights or forces are now considered except those due to this horizontal force. This force is considered thus apart from all others because it is a force between two joints, and must enable us to obtain a pair of mutually reciprocal figures, such as weights and other applied forces seldom give.

It is seen that the force between these joints might be supposed to be caused by a tie joining these points; and in general it may be stated that the diagram of forces due to any cambering or stress induced in a frame by "keying" pieces, is mutually reciprocal to the frame diagram.

Let any piece of the frame be denoted by the letters in the spaces on each side

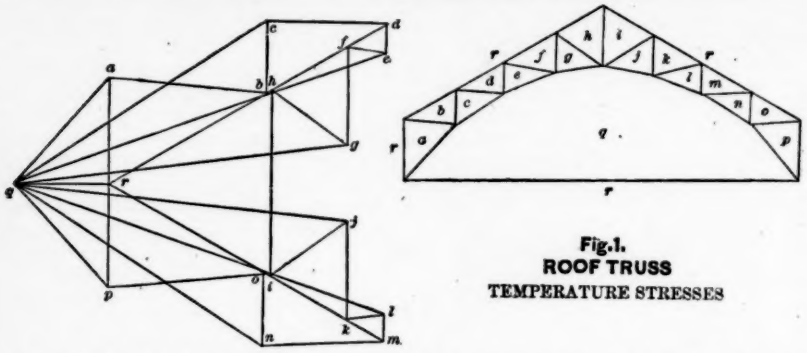


Fig. 1.
ROOF TRUSS
TEMPERATURE STRESSES

of it; thus the pieces of the lower chord are *qa*, *qc*, *qe*, etc.; and those of the upper chord are *rb*, *rd*, etc., while *ab*, *bc*, etc., are pieces of the bracing, and *qr* is the tie whose tension produces the stress under consideration.

In the force diagram upon the left, let *qr* represent, on some assumed scale of tons to the inch, the tension in the piece *qr*; and complete the triangle *aqr* with its sides parallel to the pieces which converge to the joint *aqr*; then must this triangle represent the forces which are in equilibrium at that joint. Next, with *ar* as one side, complete the triangle *abr*, by making its sides parallel to the pieces meeting at the joint of the same name:—its sides will represent the forces in equilibrium at that joint. In a similar manner we proceed from joint to joint, using the stresses already obtained in determining those at the successive joints.

It is not possible to determine in general more than two unknown stresses in passing to a new joint, unless aided by some considerations of symmetry which may exist at such a joint as *ghijq*.

Now from the left hand figure as a frame diagram, in which stresses are induced by causing tension in the tie *qr*, we can construct the right hand figure as a force diagram, but it must be noticed in that case that *rb*, *rh*, *rf*, *rd* are separate and distinct pieces meeting at the joint *r*, although they all lie in the same right line, and that the same is true along the line *o i k m*.

One or two considerations of a general nature should be recalled in this connection.

A polygon encloses the space *q*; in the reciprocal figure the lines parallel to its sides must all diverge from the point *q*: and if the upper chord had been a polygon, instead of being of uniform slope, the lines parallel to its sides would diverge from the point *r*. As it is, *ra*, *rb*, *rd*, *rm* etc., form the rays of such a pencil, in which several rays are superposed one upon another.

The determination of the question as to whether the stress in a given piece is tension or compression is effected by following the polygon for any joint completely around and noting whether the forces act toward or from the joint: *e.g.* at the point *fghrf*, from following the diagrams of preceding joints in the manner stated, it will be found that *fg* is under tension, and acts from the joint; consequently, *gh* which acts toward the joint is under compression, as are also the two remaining pieces. Hence if the tension in the tie *qr* be replaced by an equal compression in a part, tending to move the lower extremities of the roof from each other, the sign of every stress in the roof will be changed, but the numerical amount will remain unchanged, and no change will be made in the force diagram.

ROOF TRUSS.

As another example let us take a roof truss represented in Fig. 2, acted upon by the equal weights *fe*, *ed*, *dd'*, etc. Suppose that the effect of the wind against the right hand side of the truss is such as to cause a deviation of the force applied at the joint *a'b'e'f'* of the amount indicated in the figure. Such a deviation may of course occur at several joints of a roof, but the treatment of

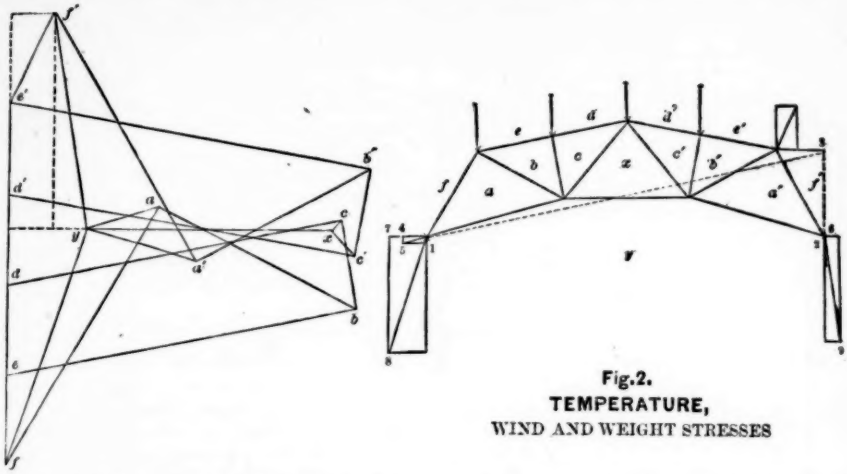


Fig. 2.
TEMPERATURE,
WIND AND WEIGHT STRESSES

the single joint at which the force of the wind is, in this case, principally concentrated, will sufficiently indicate the method to be employed in more intricate examples.

Suppose that this pressure of the wind is sustained by the left abutment. The manner in which it is really sustained depends upon the method by which the roof is fixed to the walls.

This horizontal pressure of the wind is not directly opposed to the thrust of the left abutment, consequently a couple is brought into play by these forces, whose effect is to transfer a part of the weight from the right to the left abutment. To compute the amount of this effect, draw an horizontal line through this joint (or in case the wind acts at several joints the horizontal line has to be drawn through the center of action of the wind pressure) and prolong it until it intersects the vertical at the right abutment at 3. Let 14 be equal to the pressure of the wind. Join 13 and prolong 13 until it intersects the vertical through 4 at 5, then is 45 the amount by which the weight upon the left abutment is increased, and that upon the left abutment decreased. For, let $k.\ \overline{14}=\overline{12}$. then $k.\ \overline{45}=\overline{23}$. Now the couple due to the wind $=\overline{23}\cdot\overline{14}$ but $k.\ \overline{23}.\ \overline{14}=\overline{12}.\ \overline{23}=k.\ \overline{12}.\ \overline{45}$, $\therefore\ \overline{23}.\ \overline{14}=\overline{12}.\ \overline{45}$. The right hand side of this last equation is the couple equivalent to the wind couple, having the arm 12 and a pair of equal and opposite forces represented by 45. Let 45 be added to half the weight of the symmetrical loading upon the roof to obtain the vertical reaction of the left abutment, and subtracted from the same quantity for the vertical reaction of the right abutment.

If any doubt occurs as to the manner in which the wind pressure is distributed between the abutments that distribution should be adopted which will cause the greatest stresses upon the pieces, or, as it may be stated in better terms, each piece should be proportioned to bear the greatest stress which any distribution of that pressure can cause.

Let us suppose that a horizontal compression is exerted upon the truss due to temperature or other cause, and represented by the width 26 of the rectangle at the right abutment, then the reaction at that point is the resultant 92 of this compression and the vertical reaction; while at the left abutment the total horizontal reaction 71 is the sum of this compression and the resistance called into action by the wind, giving 81 as the resultant reaction at the left abutment.

Now, using a scale of force twice that just employed, for the sake of greater convenience and accuracy, construct *defyf'e'd'* the polygon of the applied forces; and proceed to construct as in Fig. 1 the polygons of forces for each of the joints. The accuracy of the construction will be tested by the closing

of the figure at the completion of the process.

The force diagram at the left is the reciprocal figure of the diagram of the frame and applied forces at the right, but the figure at the right is not the reciprocal of that at the left since it is not a closed figure with at least three lines meeting at each intersection.

BRIDGE TRUSS.

As a further example take the bridge truss shown in Fig. 3, which is represented as of disproportionate depth in order to fit the diagram to the size of the page. The method employed is a simplification of that given by Mr. Charles H. Tutton on page 385, vol. XVII of this Magazine.

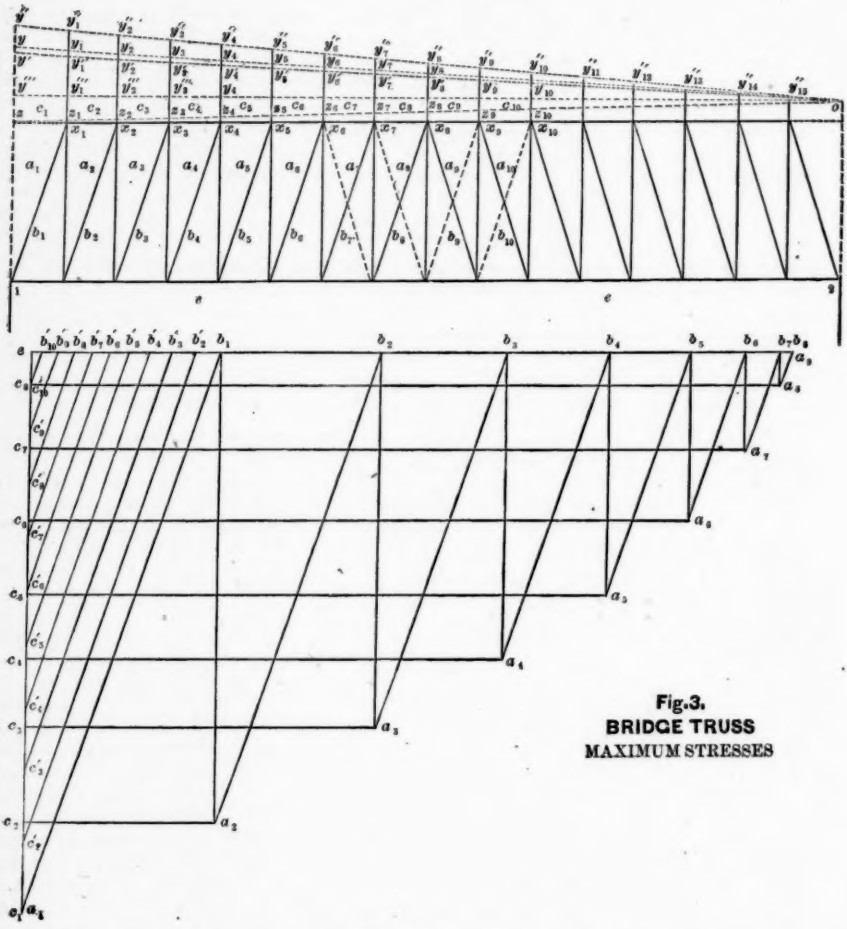


Fig.3.
BRIDGE TRUSS
MAXIMUM STRESSES

Let us suppose the dead load of the bridge itself to consist of a series of equal weights w, applied at the upper joints x_1, x_2, etc., of the bridge. Let each of these weights when laid off to scale be represented by the length of $zy'''=w$, then the horizontal lines xx and $y'''o$ include between them ordinates which represent these weights.

Let the live load consist of one or more locomotives which stand at the joints x_1 and x_2, and a uniform train of

cars which covers the remaining joints. Let the load at each joint due to the cars be represented by $y'''y'=w'$, and the excess above this of the load at each of the joints covered by the locomotives be represented by $y'y''=w''$, $\therefore\ w+w'+w''=c_1c_2=zy''=c_3c_3$ is the load at x_1 and at x_2, and $w+w'=c_3c_4=zy'$ is the load at x_3 and at each of the remaining joints.

Draw $y'o$, $y''o$ and zo, then is $z_1y_1''=\frac{1}{10}zy''$ that part of the load at x_1 which is sustained at the left abutment, as appears from the principle of the lever. Again $z_2y_2''=\frac{1}{10}zy''$ is that part of the load at x_2 sustained by the same abutment, and $z_2y_2'=\frac{1}{10}zy'$ is a similar part of load at x_2. Let the sum of these weights sustained by the left abutment be obtained; it is c_1e upon the lower figure. Upon c_1e lay off $c_1c_2=w+w'+w''$, $c_2c_3=w+w'+w''$, $c_3c_4=w+w'$, etc., equal to the loads applied at x_1, x_2, etc. We are now prepared to construct a diagram of forces which shall give the stresses in the various pieces under this assumed loading. Before constructing such a diagram, we wish to show that the assumed position of the load causes greater stresses in the chords of the bridge than any other possible position. The demonstration is quoted nearly verbatim from Rankine's Applied Mechanics, and though not strictly applicable to the case in hand, since it refers to a uniformly distributed load, it is substantially true for the loading supposed, when the excess of weight in the locomotives is not greater than occurs in practice.

"For a given intensity of load per unit of length, a uniform load over the whole span produces a greater moment of flexure at each cross section than any partial load."

"Call the extremities of the span 1 and 2, and any intermediate cross section 3. Then for a uniform load, the moment of flexure at 3 is an upward moment, being equal to the upward moment of the supporting force at either 1 or 2 relatively to 3, minus the downward moment of the uniform load between that end and 3. A partial load is produced by removing the uniform load from part of the span, situated either between 1 and 3, between 2 and 3, or at both sides of 3. First, let the load be removed from any part of the span between 1 and 3. Then the downward moment, relatively to 3, of the load between 2 and 3 is unaltered, and the upward moment, relatively to 3, of the supporting force at 2 is diminished in consequence of the diminution of the force; therefore the moment of flexure is diminished. A similar demonstration applies to the case in which the load is removed from a part of the span between 2 and 3; and the combined effect of those two operations takes place when the load is removed from portions of the span lying at both sides of 3; so that the removal of the load from any portion of the beam diminishes the moment of flexure at each point."

The stress upon a chord multiplied by the height of the truss is equal to the moment of flexure; hence in a truss of uniform height the stresses upon the chords are proportional to the moments of flexure, and when one has its greatest value the other has also.

The sides of the triangle c_1eb_1 represents the forces in equilibrium at the joint c_1eb_1 at the left abutment 1. The polygon $c_3c_1b_1a_3c_3$ represents the forces in equilibrium at the joint of the same name, *i.e.*, at the joint x_1. The forces at the other joints are found in a similar manner.

It is unnecessary to complete the figure above e unless to check the process. The stresses obtained for the corresponding pieces in the right half of the truss would, upon completing the diagram, be found to be slightly less than those already determined because there are no locomotives at the right. The greatest stresses upon the pieces of the lower chord are eb_3, eb_2, etc., and on the upper chord are a_3c_3, a_2c_2, etc.

To determine the greatest stress upon the pieces of the bracing (posts and ties) it is necessary to find what distribution of loading causes the greatest shearing force at each joint, since the shearing forces are held in equilibrium by the bracing. We again quote nearly word for word from Rankine's Applied Mechanics.

"For a given intensity of load per unit of length, the greatest shearing force at any given cross-section in a span takes place when the longer of the two parts into which that section divides the span is loaded, and the shorter unloaded."

"Call the extremities of the span, as before, 1 and 2, and the given cross-section 3; and let 13 be the longer part, and 23 the shorter part of the span. In the first place, let 13 be loaded and 23 unloaded. Then the shearing force at 3 is equal to the supporting force at 2, and consists of a tendency of 23 to slide upwards relatively to 13. The load may be altered either by putting weight between 2 and 3, or by removing weight between 1 and 3. If any weight be put between 2 and 3, a force equal to *part* of that weight is added to the supporting force at 2, and, therefore, to the shearing force at 3; but at the same time a force equal to the *whole* of that weight is taken away from that shearing force; therefore the shearing force at 3 is diminished by this alteration of the load. If weight be removed from the load between 1 and 2, the shearing force at 3 is diminished also, because of the diminution of the supporting force at 2. Therefore any alteration from that distribution of load in which the longer segment 13 is loaded, and the shorter segment 23 is unloaded, diminishes the shearing force at 3."

The shearing force at any point is the resultant vertical force at that point, and can be computed by subtracting from the weight which rests upon either abutment the sum of all the weights between that point and the abutment, *i.e.*, by taking the algebraic sum of all the external forces acting upon the truss from either extremity to the point in question; the reaction of the abutment is, of course, one of these external forces.

The greatest stress upon the brace a_1b_1 is that already found, while x_1 is loaded with the live load.

If the live load be moved to the right so that no live load rests upon x_1, and the locomotives rest upon x_2 and x_3, the pieces b_1a_2 and a_2b_2 will sustain their greatest stress. To find the shear at x_2 in that case, we notice that the change in position of the live load has changed the reaction c_1e of the left abutment by the following amounts: the reaction has been diminished by the quantity $y_1'''y_1'' = \frac{1}{16}(w'+w'')$, since the load at x_1 has been removed, and it has been increased by $y_2'y_2'' = \frac{3}{16}w''$, since x_2 is loaded more heavily than before, therefore the reaction of the abutment has on the whole been decreased by the total amount $\frac{1}{16}(15w'+2w'')$.

Now the shear at x_2 is this reaction diminished by the load w at x_2. In order to construct it, draw yy_{1v}'' parallel to $y'o$, then $yy' = \frac{2}{16}w''$. $\therefore$ Shear at $x_2 = ec_1 - w - \frac{1}{16}(15w'+2w'') = ec_1 - x_1y_1$. Lay off $c_1c_2' = x_1y_1$, then the shear at $x_2 = ec_2'$ = the greatest stress in the brace b_1a_2; and $b_2'c_2'$ = the greatest stress in a_2b_2.

Again, to find the greatest shear at x_2 when the live load has moved one panel further to the right, we have the equation: Shear at $x_2 = ec_2' - w - \frac{1}{16}(w'+w'') + \frac{2}{16}w'' = ec_2' - w - \frac{1}{16}(14w'+2w'') = ec_2' - x_2y_2$. Lay off $c_2'c_2'' = x_2y_2$, then the shear at $x_2 = ec_2''$, which is the greatest stress in the piece b_2a_2, while $b_2'c_2'$ is the greatest stress in a_2b_2.

In similar manner lay off, $c_2'c_4' = x_3y_3$, $c_4'c_5' = x_4y_4$, etc., until the whole of the original reaction ec_1 of the abutment is exhausted, then are ec_1, ec_2', ec_2'', ec_4', etc., the successive shearing stresses at the end of the load, *i.e.* the greatest shearing stresses, and consequently these stresses are the greatest stresses on the successive vertical members of the bracing, while c_1b_1, $c_2'b_2'$, $c_2''b_2''$, etc., are the greatest stresses on the successive inclined members of the bracing.

Had the greater load, such as the locomotives, extended over a larger number of panels, the line $y_1y_2y_3$ would have cut off a larger fraction of $y'y''$. Suppose, for instance, that the locomotives had covered the joints x_2x_3 inclusive, then the line y_1y_2 would have passed through y_3'', and been parallel to its present position. In that case the ordinates x_1y_1, x_2y_2 would have been successively subtracted from the reaction of the abutment due to a live load covering every joint, in order to obtain the shearing forces, just as at present, until we arrive at x_3, after which it would be necessary to subtract the ordinates x_3y_3'', x_4y_4'', etc. The counter braces are drawn with broken lines. Two counters are necessary on each side of the middle under the kind of loading which we have supposed. It is convenient, and avoids confusion in lettering the diagram to let a_2b_2, for instance, denote the principal or counter indifferently, as both are not subject to stress at the same time.

The devices here used can be applied

to a variety of cases in which the loading is not distributed in so simple a manner as in this case.

IN GENERAL.

This method permits the determination of the stresses in any frame when we know the relative position of its pieces and the applied forces, provided the disposition of the pieces is such as to admit of a determination of the stresses.

The determination of what the applied forces are in case of a continuous girder or arch is a matter of some complexity, depending upon the elasticity of the materials employed, and the method in its present form affords little assistance in finding them.

Some authors have applied the method to find the stresses induced in the various pieces of a frame by a single force first applied at one joint, and then at another, and so on, and, finally, to find the stresses induced by the action of several simultaneous forces, by taking the algebraic sum of their separate effects. This is theoretically correct but laborious in practice in ordinary cases. Usually, some supposition respecting the applied forces can be made from which the results of all the other suppositions which must be made, can be derived with small labor. The bridge truss treated was a remarkable case in point.

IMPROVEMENT OF THE SOUTH PASS OF THE MISSISSIPPI.*

BY E. L. CORTHELL, Resident Engineer.

IN regard to the depth of channel that exists at present through the jetties, it will be sufficient to state that on the first day of November, this year, the steamship City of Bristol, of the New York and Liverpool Inman Line, passed through the jetties without detention and without touching. Her draught was 21 feet and 8 inches, the tide at the time was 2½ inches below "average flood tide," which is the plane of reference established by the United States engineers.

It may not be generally understood, that the delay in obtaining the deeper channels demanded by the contract between Capt. Eads and the United States Government has been caused mainly by the difficulties which have been met with in deepening the river shoal at the head of the Passes.

First, there was not a full knowledge and comprehension of all the conditions existing, nor of their relation to each other, and to the channel, which it was necessary to make into South Pass; secondly, it was necessary that the works should be partly tentative in their nature, and this fact demanded considerable time in which to observe their effects; thirdly, many months were consumed in building these extensive works which would control the whole volume of the river. All these causes have delayed the formation of the channel through the bar at the Gulf, where the jetties are located. Without entering beyond a few explanations into the discussion of this most difficult and interesting problem of river hydraulics, on which a treatise could well be written, we simply refer to the fact of its existence and of its intimate relation to the channel through the jetties.

There are conditions existing of so subtle a nature, with relations so obscure and yet so entirely dependent upon each other; there are hidden causes, that operate so quietly and easily, to change existing conditions and to destroy the equilibrium of natural forces; there are at times such unlooked-for results from the works constructed, and there are so many occult principles and unknown laws in the flow of water moving in immense volumes that are elucidated by these results, that in the end, one of the most useful and interesting histories may be written, that will record in detail all the steps which have been taken to persuade the waters of the great river to go where man directs.

With all the interest that may attach to the subject in the mind of a disinterested person, who views it all calmly

* A letter to the *Providence Journal.*

from a distance, yet by those who have not only their reputations but their financial prospects at stake in the success of these jetties, it is deeply regretted that the completion of the work and the securing of the maximum channel has been delayed so seriously, and the success of the enterprise endangered by what appears now to have been false economy, which decided upon the improvement of the South Pass rather than of the South-west Pass.

To improve, by the force of river currents, a shoal which lies as a great middle ground between the two main outlets of the river, is one of the most difficult of undertakings. To induce the river to leave the channels in which it has flowed for ages, and to seek a new and untried one, is in opposition to its conservative nature; it hesitates long, and is very loth to leave the old ways for the new.

It is necessary to show this almost sentient monster that there are sound hydraulic reasons for changing its ancient habits.

All the works that are built in the funnel-shaped mouth of South Pass, have a tendency to throw a part of its volume into the two large passes, which stand right there with their great, hungry mouths to take every drop that comes within their reach.

To avoid this result of our works, it has become necessary to hold the sections of these passes *in statu quo* by laying on their beds a sill of willow mattresses, from our works, at the head of South Pass, to the east and west banks of the river.

The total length of these sills is about one mile and a quarter. They are simply the foundations of more extensive works of the same kind, by which we expect to control, slowly but surely, the whole river volume, and draw from it, what South Pass needs to obtain and maintain, a channel, thirty feet deep and 350 feet wide, to the deep waters of the Gulf of Mexico.

Another subject which we wish to refer to, especially, is the "re-formation." of the bar in front of the jetties; that "re-formation" that has been held up by some of our friends, as the great, final and grand obstacle to our success.

In the earlier stages of the work, it was the impossibility of securing even a twenty foot channel that troubled them; the absurdity of our expecting any such result was so clearly patent to their minds that they thought their argument unanswerable, and convinced themselves any rate of our prospective failure; but the 20 foot channel came, then the 21, now nearly the 22, and through the jetties a broad, deep channel from 24 feet to 95 feet deep, has made its appearance, and has thus steadily and surely furnished us facts for our arguments that 30 feet would surely come.

Of late we hear but little of the absurdity of our intention to obtain a deep channel; but now it is *that bar*, that *new* bar that is to form so rapidly, close up the jetty outlet and bring disaster upon the whole enterprise!

Well, it is now two years and a half since we laid the first willow mattress on the South Pass bar. The same enormous volume of mud and water has flowed out through the jetties that formerly spread out like an open fan a mile and a half in width, and in addition to it nearly four million cubic yards of sand and clay have been excavated from the shoal at the head of the Pass and from the channel between the jetties and thrown out into the gulf.

The arguments on either side have facts now, or the absence of them, to prove or disprove the conflicting theories. The time has come when the American people who are to pay for this great work can demand the facts, that they may know whether all the money expended and to be expended, is irretrievably lost in a mud bank that will pay no dividend to its depositors, or will result in a deep and permanent outlet for the Mississippi Valley.

In the limited space allowed us in your crowded columns, we cannot give all the details connected with the facts in our possession. If we could do so, and could illustrate them by maps and diagrams, we could show to the satisfaction of the most obstinent opponent of the jetties that the re-formation of the bar, as prophesied, is a myth. We will briefly allude to the facts and give general results only.

In May, 1875, the United States Coast Survey, under the direction of Mr. H. L. Marindin, one of the most efficient and

careful assistants in the department, made an accurate and detailed survey of the South Pass bar. This survey extended into the Gulf about half a mile from the outer crest of the bar.

In October of this year, (1877,) we made a survey covering the same ground. The lines of soundings were run in the same manner as those of Marindin. The general and detailed plan was similar to his. The work was plotted on the same scale. His soundings were increased by one foot and eight-tenths, to bring his plane of reference up to ours.

The whole area in front of the jetties was then divided into squares of one hundred feet. We took it for granted, that between any two soundings of his and also of ours, the slope was uniform. Depths were thus obtained for each corner of each square.

A skeleton was made, on which these squares were drawn; the calculated depth at each intersection was put down in one color for Marindin's survey, and in another color for ours, and we then had the data carefully arranged for calculations by the prismoidal formula. This plan was adopted on account of the unequal spacing of the soundings and, while it is open to the charge of interpolation, it gives under the circumstances the most accurate results.

The depths having thus been put down, the skeleton was subdived into larger areas and figures. Using the one thousand feet, between the present end of the jetties, as the base of a rectangle, the jetty lines were produced into the Gulf 2500 feet.

This figure was divided again into smaller rectangles of 1000 feet by 500 feet.

The large rectangle was enlarged by adding 1000 feet to the width at the outer end (500 feet on each side) but retaining nearly the same width at the end of the jetties. Then a larger figure still was drawn in the same way, and so on, until the final figure embraced the fan-shaped area of the discharge of sedimentary matter issuing from the jetties during high river.

The calculations show that over the rectangle (1000 feet by 500 feet) lying immediately in front of the jetties and adjacent to their ends, there has been an average deepening of four feet and two inches.

This is the identical ground over which the bar re-formation was to take place so rapidly. The rectangle first spoken of, namely, 1000 feet by 2500 feet, we will call A; the next larger figure B, the next C, and so on, each one embracing all the preceding. The result of these calculations is given in the following table:

Letter of Combination.	Area of Combination in square feet.	Mean gain in depth on Combination since May, 1875, in feet.	Quantity of material scoured in Combination since May, 1875, in cubic yards.
A.....	2,500,000	0.878	81,341
B.....	4,000,000	0.727	107,690
C.....	5,300,000	0.996	195,609
D.....	6,520,000	1.144	276,328
E.....	7,770,000	1.166	335,572

A map exhibiting graphically the changes that have taken place, shows clearly that there are well defined areas of scour entirely across the whole mile and a half investigated, and at right angles to the end of the jetties.

We do not need to go beyond this map to prove the existence of a well marked and almost constant littoral or shore current of salt water, sweeping under the fresh water, and not only carrying to one side the vast amount of sediment thrown out of the jetties, but even digging down and cutting into and carrying off much of the original outer slope of the bar.

The surveys and calculations made recently by Capt. M. R. Brown, U. S. Engineers, who is detailed by the Secretary of War to inspect the jetties for the Government, corroborate the statements we have made.

In his report of July, 1877, to the Secretary of War, he compares his survey of June, 1876, with that of June 1877.

We quote some passages that refer to this special subject, and which are found in pages 26, 27 and 28:

"On sheet No. 4 will be found the results of a survey, in June 20th, to June 22d 1877, of a mile or more beyond the

ends of the jetties, and for a considerable space on either side."

"The subject of fill and scour beyond the ends of the jetties has occupied so much attention that I have carefully compared the results of the survey of June, 1876, with that of June, 1877, and for the purpose have divided the whole area, comparable by means of the two charts, into twenty-one divisions."

"The fan-shaped areas are, of course, those of the most pressing interest when investigating the influences of changes in the immediate future of commerce."

"Taking into account all the divisions except 1, 7, 13 and 21, we find that the scour in the year was 1,145,976 cubic yards, equivalent to a scour of 1 3109-10,000th feet, or 1 foot, 3.7 inches over this latter area."

MAINTENANCE OF THE PAVEMENTS IN PARIS.

Translated from "Annales des Ponts et Chaussées."

For cleaning streets, machine sweepers are employed drawn by a single horse, cleaning about 5,000 square meters an hour.

The cost of keeping in repair is quite different for the different avenues; for the Rue Lafayette it is 16.08 francs.

The asphalt roadways have a joint area of 225,120 square meters, to which should be added about 34,000 square meters for the walks through the Macadamized streets. The price of construction varies from twelve to fifteen francs per square meter.

The repairing done by contract for 1.10 francs per square meter per year for the roadways, and 1.70 francs for the walks.

The mean cost of repairing roadways in Paris, which was 1.08 francs in 1870, has been reduced to 0.82 francs. This reduction is due especially to a change in many places from Macadam to paved roadways. The mean cost of repairing pavement never exceeds 0.60 franc, while Macadam roadways cost 1.80 francs per square meter. The latter should therefore be replaced, except where they serve as promenades and ornaments, as in the boulevards and avenues.

THE following estimates are extracted from a recent report to the Municipal Council of Paris by M. Watel.

The number of vehicles which pass daily through some of the principal thoroughfares of the city have been ascertained to be as follows:

Boulevard de Sebastopol.....	11,602
Avenue des Champs Elysees...	11,734
Rue de Rivoli.....	13,898
Rue Royale.....	16,177
Boulevard des Capucines.....	19,043

The paved roadways have an aggregate total area of 5,458,000 square meters; their maintenance requires the constant service of 431 men (*cantonniers*). The cost per square meter varies from 15.90 francs to 20.40 francs according to the gauge (.10 to .16 meter).

The cost of hand labor in keeping the pavements in order is 0.154 francs per square meter.

The Macadamized roadways cover an area which, although less than in 1870, is still 1,900,000 square meters. The number of *cantonniers* required for their maintenance is 965.

The steam rollers employed weigh about thirty tons each. The rolling is generally completed in a single night.

THE statistics of the loss of human life in the mining of anthracite coal, as gathered by the Pennsylvania mine inspectors, show the following results:—In Shamokin district, ratio of coal produced per life lost, 86,711; in lower Schuylkill district, 107,078; in southern district of Luzerne and carbon counties, 94,679; in middle district of Luzerne and carbon counties, 83,916; in Wyoming Eastern district, 110,511; average, 96,579; in English bituminous mines, 1875, 118,730; in Nova Scotia mines, 1874, 135,063; in Ohio mines, 1875, 142,352; average in Pennsylvania, anthracite mines, 1875, 92,437.

FORCE, MOMENTUM AND VIS VIVA.

By DE VOLSON WOOD, C. E., M. A.

Written for Van Nostrand's Magazine.

Volumes have been written upon the subjects which head this article, and probably nothing new remains to be said in regard to them. But these pages bear evidence to the fact that writers still differ in regard to their essential meaning, or at least, in regard to their true measure, and it is the privilege of each to defend his position, although he may use arguments that are well nigh worn out.

There are two ways of considering these quantities; one in the light of analysis, and the other in the light of physics; but the results of both, if correct, must necessarily agree. There is an advantage in considering them analytically; for if the equations which represent the quantities are proved and accepted, we have a sure foundation on which to stand, and if any discrepancy between them and observed phenomenon appears to arise we may be certain that our conception of the physical part is incorrect.

All that is necessary to be said in explaining these quantities may be said in a few words. Discussions in regard to them are prolonged because attempts are made to apply them to a great variety of cases, and to give special explanations for each case. We know force only by its effects. Laplace says "The nature of that singular modification, by which a body is transported from one place to another, is now, and always will be, unknown; it is denoted by the name of *Force*." Gravity is one of the forces of nature. If we attempt to hold a body at rest against its action, we experience a pull equal to the weight of the body. If the body is permitted to fall freely, it will have a certain definite acceleration. Here are two distinct results, which give rise to two distinct measures of the same cause. Let any other force, as the pull of a horse, or the force of electricity, act upon a body, it will exert a certain definite pull if the body be prevented from moving, or, if the body be left free to move this force will produce a certain definite acceleration during each instant of its action.

I will use the term *pull* or pressure in a general sense. Let W be the measure of the pull of gravity upon a body, and g the acceleration which it produces in a second of time; similarly let F be the constant pull exerted by any other force upon the same body, and f the acceleration per second which it would produce upon the same body if perfectly free to move under its action. The ratio of these pulls we assume to be the same as the ratio of their accelerations, for like causes produce like effects, and hence we at once have

$$\frac{F}{W}=\frac{f}{g}, \quad . \quad . \quad . \quad . \quad (1)$$

This is the *fundamental* equation of dynamical science, and all other equations of direct motion follow from it. Each member of this equation is an abstract quantity. We may operate upon it algebraically, but having changed its form, and hence the relation of the parts to each other, it will remain to give a proper interpretation to the resulting equation. First, then, find the value of F. We have at once

$$F=W\frac{f}{g}. \quad . \quad . \quad . \quad (2)$$

But the members are no longer abstract quantities. The left member as we said at the outset represents the pull, which we will for convenience *call pounds*. The first member being pounds the second member must also be pounds; or since W, in the second member is pounds, and $\frac{f}{g}$ is an abstract quantity, the second member must be pounds, and hence the first member must also be of the same denomination.

But another transformation gives us

$$F=\frac{W}{g}f,$$

in which $\frac{W}{g}$ is called the mass,—or the measure of the mass of a body whose weight is W at a place where the acceleration due to gravity is g, and is repre-

sented by M. The quantity M is the g^{th} part of the weight of the body. The equation now becomes

$$F=Mf, \quad . \quad . \quad . \quad (3)$$

In this equation the first member is the statical measure of a force; the second member the dynamical measure. The first member is *pounds*, the second member is an equivalent measure of these pounds. The second member has been called by Gauss *the absolute measure of force*, and it is often called by that name at the present time. It is not necessary that it should have a name, but it is quite proper that it should have one. I see that Professor Skinner, in the preceding volume of this Magazine, page 422, proposes to call it *tend*. I trust that the *tend*ency will be to oppose the introduction of such technical names unless they are absolutely necessary. If the three names at the head of this article have caused discussions for two centuries or so, and, as is now admitted, are not yet fully understood, what would become of the science of mechanics if three more abstract terms were introduced! Professor Skinner also remarks on the same page that "As to practical convenience there may sometimes be advantage in using this unit of force." Now I remark that it is absolutely necessary that it (the absolute unit) should be used. Were we unable to measure the pounds of pressure (or pull) which produces motion in a body by the elements which enter into that motion, Dynamics would be a sickly science, if indeed it could exist as such. Whether we give a name to the second member of equation (3), or not, the fact that *the mass of a body into its acceleration measures at each instant the pounds pressure exerted upon a body*, enables us to establish an equation—a fundamental equation—from which all others in regard to motion, work and momentum are derived. These equations are, 1st:

$$F=M\frac{d^2s}{dt^2};$$

multiplying by dt and integrating and we have a second, or

$$\int Fdt=M\frac{ds}{dt}=MV;$$

or, multiplying by ds, we have the third, or

$$\int Fds=\tfrac{1}{2}M\frac{ds^2}{dt^2}=\tfrac{1}{2}MV^2,$$

An interpretation of these equations, covers the whole ground of the controversy. But, in order to present this subject without the use of the Calculus, we will consider *constant* forces. It has been proved by Atwood's machine (or in some other way) that

$$f=\frac{V}{t},$$

in which V is the velocity produced in a time t by a constant acceleration f. Substituting this value in equation (3) gives

$$F=M\frac{V}{t},$$

or

$$Ft=MV. \quad . \quad . \quad . \quad (4)$$

The second member of this equation is called *momentum*. But what does that mean? What is its office? What is its function in the physical world? We will answer these questions affirmatively and negatively, and somewhat in detail.

1st. Its office cannot be determined from its name. That may be used as a mere convenience. It may be a name given to the product MV; or to an effect produced. If the latter, what effect?

2d. It cannot be determined from the product MV. A body whose mass is M may have a velocity V, but we cannot tell what function the product of these quantities is without considering what produces the effect.

3d. Its office can be determined only in connection with the first number of equation (4). From that we find, that

4th. *It is the measure of the effect of a moving force F acting upon a body during a time t.* For the sake of brevity I call this a *time-effect*, a term which is partly descriptive of the first member of the equation. This effect is independent of the space passed over, except that space is implicitly involved in velocity.

5th. It is not the measure of force.

6th. Momentum may be made to measure a force—numerically. Observe, I do not here say the momentum of the body, but, as will soon appear a certain momentum, a restricted momentum.

Some of these statements admit of further explanations. By moving force I mean that part of the acting forces

which produce motion only. Thus a body is acted upon by a constant pull, but there may be resistances of friction, or of the air, or the motion may be opposed by a pull in the opposite direction. If the force which produces motion only be removed, all the other forces will be in equilibrium. If all the forces which are in equilibrium among themselves be removed, then will the measure of the *time-effect* of the remaining force be the momentum. But just here we meet an objection by Professor Skinner, in the preceding volume of this Magazine, page 426, where it is claimed that "Writers have generally agreed to measure the *effect* of a force by work." I confess that I am ignorant of such an agreement. They say, at the outset, that *force is measured by its effects.* What are its effects? One of its effects is to produce pressure between two bodies; another is to produce motion; another, to do work; another, to produce energy; another, to produce tension in elastic bodies. That writer further adds: "It seems to me that we need in mechanics a name which shall apply to the *exertions of a force during time*, whether this produces apparent motion of masses or not," and suggests the word *toil.* First, I inquire, what is meant by a force acting during time without producing motion. Is mere pressure exerted during time anything but pressure? Remove this ambiguity from the condition, and the propriety of adopting the same term as the one commonly used, will be apparent. I refer to the well-known term *Momentum.* The proposed *toil* can find no place in mechanics without expelling the present occupant.

I am unable to determine Professor Skinner's position. For instance, on page 128 of the preceding volume, in regard to the unit of momentum, he raises the question, "If a body equal to 10 units of mass have a velocity of 10 feet per second, its momentum will be equal to $10\times10=100$. But 100 what? If *momentum is quantity of motion* it certainly cannot be 100 pounds, and it is quite as difficult to say that this body has a *quantity* of *motion* equal to 100 feet." But on page 132 he says: "The unit of *momentum*, then, is a force of pressure equal to one pound." I supposed that this was an over-sight, but I find on page 133 the following: "we saw that momentum, which is equal to MV, represents the number of pounds pressure which the mass M with the velocity V is capable of exerting under a certain arbitrary condition." Here the mass, or body not the force, exerts a pressure, and it equals a certain number of pounds; but this position appears to be demolished in his article in the October number of the Magazine, pages 321–327. Again, on page 235, we find "its momentum will be $1441\frac{2}{3}$ pounds;" on page 237, "You may say, if you please, that it has a negative momentum equal to $441\frac{2}{3}$ pounds;" page 240, "We have seen, first, that the unit of momentum is simply a pound pressure, a unit which does not include the idea of motion"! From these and several other expressions, I would infer that Professor Skinner claims that momentum is pounds of pressure. But, on the contrary, we find on page 130 and in several other places it is explicitly stated that momentum is the pounds of pressure which, *acting during one second*, in an opposite direction to the motion, will bring the body to rest. If time, or the unit of time, is essential to momentum, then *the idea of motion* is involved. But, if the last definition is the correct one, what difficulty has been removed? It is simply equivalent to assuming that one momentum equals another momentum, or

$$Ft=F.\ 1\ sec.=MV, \qquad (5)$$

which is evidently correct, but, I repeat, what difficulty has been removed, or what essential principle explained? The physical quality of momentum is exactly the same as before and equally obscure; if, indeed, there is any obscurity about it. The fourth principle given above, states exactly what it is and, we hope, without ambiguity. Still, one or two remarks more in regard to it may be proper. If only a single force acts upon a body, the body must be—indeed it will be—perfectly free to move, and the relation

$$Ft=MV$$

will constantly be true. But a body, acted upon by pulling and resisting forces may move at an uniform rate; in which case the product Ft has no meaning during such uniform motion.

In regard to the unit of momentum, it may be determined from either member of equation (4). In the second member, if the unit of mass is one pound of mass, and the velocity one foot per second, then *the unit of momentum will be one pound of mass moving with a velocity of one foot per second.* Or, if a compound word be used, as is done in the case of work (I refer to "foot-pound") we would have, (*mass-pound*) (*velocity-unit*). This is sufficiently awkward to explain why such a term has not been used, if any explanation were necessary, and I have given it only for the purpose of making it correspond in form with the work-unit. But we may deduce the unit from the first member of the equation, as is done in the equation between work and energy. If F be pounds of pressure, and t seconds of time, then will the unit be *one pound pressure acting during one second;* or, adopting the compound word as before, we have for the unit a *pound-second.*

The fifth statement above, asserts that momentum is not the measure of force. To show this it is only necessary to observe, that solving equation (4) gives

$$F=\frac{MV}{t},$$

from which it appears that it is necessary to divide the momentum by the time in order to find the value of the force. But the quotient obtained by dividing the momentum by time is not momentum, but pounds, or its equivalent. We say, therefore, that *momentum never measures force.*

But if, in the preceding equation, t is unity, one second say, and the velocity generated in one second be denoted by V_1, we have

$$F=\frac{MV}{1\ sec.}=MV_1\ numerically;$$

hence, a simple interpretation of the formula gives

The measure of a constant force is numerically equal to the momentum which it is capable of producing in a free body in a unit of time.

The word *numerically* is ordinarily omitted in this rule, but we are seeking to preserve the exact relations of the quantities.

If *numerically* is omitted it should read *the mass into the acceleration,* instead of *the mass into the velocity per unit of time.*

It is well to observe that, when the force is constant $V\div 1$ *sec.* is the acceleration, in other words, the velocity produced in one second equals (numerically) the constant acceleration. Calling this f and we have

$$F=Mf$$

which is equation (3).

If the force be constantly varying, but of such intensity that it would produce a velocity of V_2 in one second provided its intensity did not change, then we have

$$F=M\frac{dV}{dt}=M\frac{V_2}{1\ sec.};\qquad (6)$$

that is, *the measure of the intensity of any force at any instant is numerically the momentum which it is capable of producing in a free mass in a second of time.*

Observe, that the measure is not the momentum which the body has, but the momentum added to (or taken from) the body each instant that is the numerical measure.

Equation (6) also shows that the measure of a force is the rate of charge of the momentum of a body. Since this expression has been criticised by Professor Skinner I emphasize, by stating that it is not merely the *numerical* but the *actual* measure. *Rate* is not an abstract ratio. The expression $\frac{dV}{df}$ is the acceleration, which we have represented by f, hence equation (6) becomes $F=Mf$ which again is equation (3).

Momentum is frequently defined as *quantity of motion.* To this, objection is made by Professor Skinner in his articles in the preceding volume, pages 136, 235, &c. Without following this writer through all his examples and arguments, we observe that there are some illustrations on page 235 which are so liable to mislead, that the errors ought to be corrected, and when corrected, will leave his position in regard to positive and negative momenta, without foundation. That writer desires to show, if I understand him, that momentum must always be considered as positive,—that there is no negative momentum. To do this, he

shows how a momentum which is considered negative may be changed to a positive one. He says, page 235, "Besides there are many ways in which the direction of motion may be changed without material loss of velocity. Suppose the mass M after the recoil (it was supposed that the mass M had struck another body and recoiled) to strike perpendicularly a fixed elastic spring. It will be thrown back again in the direction of the first motion with a little loss of velocity." For the sake of the argument, suppose that there is no loss of velocity. Then, in thi new condition, he says (correctly) that the total momentum is the sum of the two momenta; the negative having been changed to positive. But where can a *fixed* spring be found? Rest the spring against the solid earth, if you please, and will no effect be produced upon the earth by the impact of the body upon the spring? Will not that writer's own reasoning show that the momemtum imparted to the earth will be twice that of the ball thrown back, and thus the total momentum of the two masses *plus* that of the earth be the same as that of the two balls immediately after impact? They certainly will. The same result will be true of the semicircular arc which he uses. In the use of the inclined plane the momentum has been first overcome by the force of gravity, and then again restored to it during the descent. Or, in the case supposed, page 235, in which the bodies are supposed to raise vertically other weights, if the motions be traced out it will be found that everything is consistent with the algebraic idea of *plus* and *minus* momenta. He remarks on page 237 "We need not be afraid of the fact that it is possible to have an actual increase of momentum by impact." Still, I confess, we are afraid!

Now the expression *quantity of motion* may not be the best, it may even be unfortunate, but it is not necessarily false. The meaning intended to be conveyed by it may be explained just as well as if any other term were used,—as *toil* for instance. The effect of a force, acting on a free body, is to produce motion. Here motion is used in a general sense. The *rate* of motion is velocity. The *quantity* of motion is momentum. There is a wide distinction between *rate* and *quantity*. At least one English writer says momentum is *quantity of Velocity*. This, I consider, is false. Quantity of rate is absurd. We might as well say quantity of foot, quantity of minute, &c. One of the elements which enters into the measure of the motion is velocity, and the other is mass. We say in the abstract that a given force produces a definite amount of motion, but this force acting on a small mass will produce a greater velocity than if applied to a large mass. Different forces will produce different amounts of motion, that is to say different quantities of motion. We say that the intensity of heat is the amount of heat in a unit of volume, but the quantity of heat is the total heat in the body; the intensity of light is the amount of light on a unit of area, but the quantity of light is the total amount on the whole area; the intensity of gravity is the acceleration it will produce on a unit of mass, but the quantity of gravity is the total force on the body, or $W=Mg$. So the intensity of the motion is the velocity, V, and the quantity of motion is MV.

In regard to this subject the writer was criticised by Professor Raymond of West Point, in his caustic review of the writer's work on Analytical Mechanics, but these need not be considered here as I made a somewhat extended reply to them in the *Mining and Engineering Journal* of June 9th of the present year.

We now make another modification in equation (3). By means of Atwood's machine, or in some other way, it has been proved, that, for constant forces producing motion, we have

$$f=\frac{V^2}{2S}$$

in which V is the velocity produced in a space S. Substitute this in equation (3), and we have

$$F=\tfrac{1}{2}M\frac{V^2}{S};$$

hence, multiplying by S, we have

$$FS=\tfrac{1}{2}MV^2 \quad . \quad . \quad . \quad (7)$$

We now proceed to interpret this result. The first member is evident. It is the product of the constant force by the space over which it acts, and may be called briefly *space-effect*. What is the second member? We say here, as we

said in regard to momentum, that it cannot be determined by itself; it can be determined only in connection with the first member, or that member which contains the value of the force. Both members of this equation have received appropriate names. The first member is called *work*, the second, *energy*, not *potential* energy, as stated in the last lines of page 240 of the preceding volume, but *kinetic energy*. The first member is the work which the force F does in having its point of application moved over a space s in the direction of action of the force. The second member is the energy of the mass M. The energy of a body therefore, equals a *space-effect*. It equals a certain amount of work. It is stored work.

The expression for work in mechanics is derived from, or is the same as, the expression for physical work. A force may work without producing energy*. Thus, a horse, drawing a load at a uniform rate does work but produces no energy. The same is true of a locomotive drawing a train at a uniform rate. If a resistance F has been overcome through a space s work has been done regardless of the time occupied in overcoming it. Here we see an essential difference between the first members of the equations for momentum and energy. If a body has been dragged from one point directly to another, a certain amount of work has been done. Suppose, now, that we see the body in the first position on a certain day, and in the second position at any subsequent time, and that we know the resistance F which must be constantly overcome to move the body. Then will Fs have a well-known meaning, but Ft will be meaningless. The mere lapse of time accomplishes nothing. Will those who seek a common unit between work and momentum (and there are many such) show *any* connection between Fs and Ft in the above example? If the time occupied in moving the body were known, the product Ft would still have no rational meaning. Hence it is, that work is truly said to be independent of the time. If the *rate* of working be given, then time becomes as important an element as space.

When quantities differ in any essential quality, they cannot have a common unit. There is no unit common to a line and an area; or an area and a surface; or time and space; or force and momentum; or momentum and vis viva. Each must have its own unit which is necessarily some definite part of the quantity to be measured. The *pound-second* and the *foot-pound* are as unlike as the linear foot and the square foot.

We observe, further, that equations (4) and (7) are deduced on the hypothesis that the body upon which the force acts is free to move; or, what is the same thing, that the second members of those equations expresses in terms of mass and velocity, the effect of the unbalanced forces acting on the body; the former expressing the effect produced in a time t and the latter in a space s; also that the first member of equation (4) has no meaning unless the force acts on a free body, but the first member of equation (7) has two significations, one when the force acts upon a free body producing energy, the other, when it is constantly overcoming a resistance along its path. The latter, we might call *dead work*—work accomplised; the former *live work*—work which the body is yet capable of doing. Hence it is, that momentum pertains *only* to the motion of bodies, but energy both to motion and resistance. The former, therefore, for a system of bodies moving without external forces but with mutual actions, *must* be constant; and this explains why I am afraid to have an increase of momentum by impact. But the energy of a body may be expended in producing (or imparting) energy in another body, or in overcoming resistance, or, as in the case of non-elastic impact, both at the same time. Momentum is not lost, hence cannot be conserved (or in a mathematical sense is perfectly conserved), but energy, in any particular form, may be lost and hence the importance of proving that it may be conserved; that is, that in all its transmutations, nothing is lost.

In passing we note, by way of analogy, that in the transmutation of momentum, it is always transmuted (strictly speaking *transmitted*) from one mass to another. It is a molar transmutation; but in the case of energy, the transmutation may be into heat, electricity, &c.

But right here, John, who has been

* Mass energy is here referred to. The heat energy developed by friction is discarded in this discussion.

very patient up to this point, rises in his place and asks; When a problem is assigned, how shall we know which principle to use in its solution? We answer that the problem may require the use of neither, or both may be involved. If the problem is the case of the impact of free bodies, and the resulting motion is required, the problem will be solved by the principles of momentum. For, the action and reaction between the bodies during impact will act *during the same time.* They will not (generally) act through the same space. If the resulting energy is required then the principle of energy must be used in addition to that of momentum. If the body struck be held or opposed by other forces, like an anvil, resting on the earth, or a pile driven into the ground, then nothing will be determined by momentum.

Energy is essentially positive, for if the velocity be positive or negative, the expression $\frac{1}{2}MV^2$ is essentially positive; but momentum may be positive or negative since V may be *plus* or *minus.* It is an interesting fact that the first members of the equations lead to the same result. If a force in one direction be considered positive and the opposite direction negative, then will Ft be of the same sign as F, for an action cannot proceed in negative time. Time is progressive and cannot be reversed during an action. But space may be either *plus* or *minus*, and hence we have.

$$(\pm F)\times(\pm s)=+Fs=\tfrac{1}{2}MV^2.$$

Energy may numerically measure a force. Deducing the value of F from our equation we have

$$F=\tfrac{1}{2}M\frac{V^2}{s}.$$

Let the force act during one foot on a free body, producing a velocity V_2, then we have

$$F=\tfrac{1}{2}M\frac{V_2^2}{1\ ft.}=\tfrac{1}{2}MV_2, \text{ numerically;}$$

hence, *the measure of the intensity of a constant force is numerically the energy it will produce while acting through one foot of space.*

But it is proved by Atwood's machine, or otherwise, that for a constant acceleration

$$f=\frac{V^2}{2s}$$

which substituted in the preceding equation gives

$$F=Mf$$

which is equation (3) and hence no new fact has been stated by the preceding conclusion. If the force be variable, we have, by differenting both sides of the equation,

$$Fds=\tfrac{1}{2}Md\,(V^2),$$

$$F=\tfrac{1}{2}M\frac{d(V^2)}{ds}; \quad . \quad . \quad (8)$$

that is, *the measure of any force is numerically the energy that it would develop in a unit of space, if the force acted during that unit with a constant intensity.*

But this conclusion leads to no new fact. For differentiating V^2 gives

$$d(V^2)=2VdV$$

and, by substituting the value of V we have

$$\frac{d(V^2)}{ds}=2\frac{d^2s}{dt^2},$$

and, by substituting this in the preceding equation and reducing, we have

$$F=M\frac{d^2s}{dt^2}, \quad . \quad . \quad . \quad (9)$$

which is equation (3).

Equation (8) also shows that the measure of a force is the rate of change of the energy (or vis viva) of a body per foot of distance. But this is merely equation (3) under another form, as shown by the preceding reduction, and that may be reduced to equation (1). All our results, therefore, flow from equation (1), or may be traced back to that equation.

We therefore conclude, that, in *accordance with strict technical language, neither the momentum of a body nor its energy is a measure of the force which acted upon it. Or, more generally, that neither momentum nor energy is the measure of force.*

Also, that *the momentum which a force would generate in a unit of time if acting constantly upon a free body;* or *the energy which the force would develop in a unit of space if acting constantly upon a free body;* either of these *is* NUMERICALLY *equal to the pounds of force* which produces the motion.

Also, that *neither the momentum of a body, nor its energy is a measure* NUMERI-

CALLY *of the generating force, unless the body starts from rest, and in the former case the action is for one second of time, in the latter for one foot of space;* the units being assumed as a second and a foot. Also, that *the pound pressure or its equivalent is the universal measure of force.*

When there is a clear conception of the true measures of these quantities, such meaningless expressions as: the force of momentum; the force of energy; the force of a blow; the quantity of force in a moving body; the energy corresponding to a force; and the like, will disappear.

THE DIRECT PROCESS IN THE PRODUCTION OF IRON AND STEEL.*

By C. W. SIEMENS.

From "Journal of the Society of Arts."

In mixing comparatively rich iron ore in powder, with about twenty-five per cent. of its weight of pounded coal, and in exposing this mixture for some hours to the heat of a common stove or of a smith's fire, metallic iron is formed, which, on being heated to the welding point, on the same smith's hearth, may be forged into a horse-shoe of excellent quality. The admixture with the ore of some fluxing materials, such as lime or clay, will, in most cases, be of advantage to rid the iron of adherent slag.

The simplicity of this process is such that it naturally preceded the elaborate processes now in use for the production of iron and steel upon a gigantic scale; nor can it surprise us to find that attempts have been made from time to time, down to the present day, to revert to the ancient and more simple method. It can be shown that iron produced by direct process is almost chemically pure, although the ores and reducing agent employed may have contained a considerable percentage of phosphorus, sulphur, and silicon, and that, if freed from its adherent slag, it furnishes a material superior in quality and commercial value to the ordinary iron of commerce.

The practical objections to the direct process, as practised in former days, and as still used to a limited extent in the United States of America and in some European countries, are that—

1. Very rich ores only are applicable, of which about one-half is converted into iron, the remainder being lost in forming slag.

2. The fuel used is charcoal, of which between three and four tons are used in producing one ton of hammered blooms.

3. Expenditure of labor is great, being at the rate of 33 men, working 12 hours, in producing one ton of metal (see Percy). Iron produced by direct process in the Catalan forge is, therefore, expensive iron, and could not compete with iron produced by modern processes except for special purposes, such as furnishing melting material for the tool steel melter.

But, it may be asked, could the advantages of the direct process not be combined with those of modern appliances for the production of pure and intense heats, and for dealing with materials in large masses, without expenditure of manual labor, and cannot chemistry help us to larger yields and the faculty of using comparatively poor and impure ores?

A careful consideration of these questions led me to the conclusion, some years ago, that here was a promising field for the experimental metallurgist, and that I possessed some advantage over others in the use of the regenerative gas furnace as a means of producing the requisite quantity of heat without the use of charcoal and blowing apparatus. I engaged, accordingly, upon a series of experimental researches at my sample steel works, at Birmingham, and, in 1873, I had the honor of submitting the first fruits of these inquiries to the Iron and

* Paper read at the Newcastle meeting of the Iron and Steel Institute.

Steel Institute, in a paper entitled "On the Manufacture of Iron and Steel by Direct Process." Encouraged by the results I had then obtained, I ventured with some others upon some larger applications, the principal one of which has been one at Towcester, in Northamptonshire.

Viewed by the light of present experience, it would have been wiser to have fixed upon another locality, with fuel, skilled labor, and better ores within easy reach; but, in extenuation of the error committed, it may be urged that the site was fixed by force of circumstances rather than by selection, the chief temptation being an ample supply of small Northamptonshire ore at a very low cost. It was, however, soon discovered that this ore, although capable of producing iron of good quality, was too poor and irregular in quality to yield commercial results unless it was mixed with an equal weight of rich ore, such as potter mine, Spanish ore, or roll-scale, all of which, as well as the fuel, are expensive at Towcester, owing to high rates of carriage. It is in consequence of these untoward circumstances that the works at Towcester have not been completed by the addition of rolling mills, the intention being to transfer the special machinery ultimately to existing ironworks when the process has been sufficiently matured for that purpose.

The Towcester Works were visited, in the autumn of last year, by two eminent metallurgists, Professors von Tunner, of Leoban, and Akermann, of Sweden, who have published the results of their observations in separate reports. The results noted down by Mr. von Tunner are referred to by our past president, Mr. I. Lowthian Bell, in his paper on the "Separation of Carbon, &c.," which was read in March last, and will be discussed at the Newcastle meeting. The criticisms contained in these publications are conceived in the fairest possible spirit, and form indeed a most valuable record of the progress achieved up to that time, but they furnish me with an inducement to break silence sooner than I had intended, regarding the further progress which has been effected, and the conclusions I am disposed to draw from past experience regarding the direct process of the future.

The leading idea which guided me in these was to operate upon such mixtures of ores, fluxes, and reducing agents, as would, under the influence of intense heat, resolve themselves forthwith into metallic iron and a fluid cinder, differing essentially from the methods pursued by Chenat, Guilt, Blair, and others, who prepare spongy metal in the first place by a slow process, which is condensed into malleable iron or steel by after-processes, but assimilating to some extent to the process first proposed by Mr. William Clay. In my paper of 1873 I described two methods of effecting my purpose, the one by means of a stationary, and the other by means of a rotative furnace chamber, the former being applicable chiefly where comparatively rich ores are available, and the latter for such poorer ores as occur near Towcester.

At the Towcester Works three rotative furnaces have been erected, two of them with working drums, 7 feet in diameter and 9 feet in length, and the third of smaller dimensions. The gas flame both enters and passes away from the back end of the furnace, leaving the front end available for the furnace door, which is stationary. The ends of the furnace-chamber are lined with Bauxite bricks, and the circumference with ferrous oxides, resulting from a mixture of furnace cinder enriched with roll scale or calcined blackband in lumps. About 30 cwt. of ore, mixed with about 9 cwt. of small coal, having been charged into the furnace, it is made to rotate slowly for about two and a half hours, by which time the reduction of the metal should be completed, and a fluxed slag be formed of the earthly constituents containing a considerable percentage of ferrous oxide. The slag having been tapped, the heat of the furnace and the speed of rotation are increased to facilitate the formation of balls, which are in due course taken and treated in the manner to be presently described.

These balls contain on an average seventy per cent. metallic iron, and thirty per cent. of cinder, and, upon careful analysis, it is found that the particles of iron, if entirely separated from the slag, are pure metal, although the slag may contain as much as six per cent. and more of phosphoric acid, and

from one to two per cent of sulphur. In shingling those balls in the usual manner the bulk of the cinder is removed, but a sufficient residue remains to impart to the fracture a dark appearance without a sign of crystalline fracture. The metal shows, in being worked, what appears to be red shortness, but what should be termed slag shortness. In re-piling and re-heating this iron several times this defective appearance is gradually removed, and crystalline iron of great purity and toughness is produced; but a more ready mode of treatment was suggested by Mr. Samuel Lloyd, one of my co-directors in the Towcester Company, in reverting to the ancient refinery or charcoal hearth. The balls, as they came from the rotator, are placed under the shingling hammer, and beaten out into flat cakes not exceeding an inch in thickness. These are cut by shears into pieces of suitable size, and formed into blooms of about 2 cwt. each, which are consolidated under a shingling hammer and rolled into bars.

The bars have been sold in Staffordshire and Sheffield at prices varying from £7 to £9 per ton, being deemed equal to Swedish bar as regards toughness and purity.

It may therefore be asserted, as a matter of fact, that iron and steel of very high quality may be produced from ores not superior to Cleveland ores by direct process, but the question remains at what cost this conversion can be effected. The experimental works at Towcester are, unfortunately, not sufficiently complete to furnish more than the elements upon which the question of cost may be determined, the principal reasons being that the one reheating furnace and a 30 cwt. hammer at the works are not sufficient to deal with the iron produced by the three rotators, that the iron has to be finished at a rolling-mill elsewhere, and that transports weigh heavily upon the cost of production. The principal factor in the calculation of cost is unquestionably the rotator. [A table furnishes the working result of eighteen consecutive charges as taken from the charge-book.] The mixture of ore consisted, for each charge, of 12 cwt. of Towcester ore (containing about 38 per cent. metallic iron) mixed with 8 cwt. of calcined Great Fenton ore, 1 cwt. of tap cinder, 1 cwt. of limestone, and 6½ cwt. of small coal. The time occupied for each charge was three hours fifty-seven minutes, or, say four hours, and the yield of hammered blooms was on an average 6 cwt. 2 qrs. 13 lbs., whereas the metal contained in each charge amounted (by estimate) to 9 cwt., showing a loss of 25 per cent. This loss is, however, partially recovered in using a portion of the cinder again in succeeding charges, but the portion of cinder that may be used again with impunity depends upon the amount of impurities, namely, of phosphorus, sulphur, and alumina contained in the ore. The coal used in the producers amounted to two tons per ton of hammered blooms produced, and in pricing the materials used, and labor engaged upon the work, the table—prepared by the manager at the works—gives £3 8s. as the cost per ton of hammered blooms. To this must be added for repairs and general expenses, and the cost of rolling the hammered blooms into bars, which in the case of Towcester practice are very heavy, but of which an experienced iron-master would form his own estimate. The cost of working the metal in the hollow fires is also not included, and this may be taken to add from 25s. to 30s. to the ton. The refined iron so produced will, therefore, cost from £5 5s. to £5 10s. per ton.

Other tables give the analysis of irons produced from various descriptions of ores, and Kirkaldy's tests of the mechanical properties of the iron; but it should be understood that these tests were taken with a view rather to test various modes of manufacture than to show high results. Only a small proportion of the samples had been subjected to the refinery process, and the variable percentage of phosphorus may be taken really as indicative of the extent to which the cinder had been removed from the metal.

Another table gives the analysis of slags produced in the process. These are, no doubt, rich in iron, but it must be remembered that in the case of comparatively pure ore, they can be used almost entirely in succeeding charges, and that in the case of ores containing much sulphur and phosphorus, they are the recipients of those impurities—in the same

way as the puddling cinder carries off the same impurities in the puddling furnace—and thus serve a useful end.

If rich ores, such as hematites, are available, it is more advantageous to use a stationary furnace, and to modify the process as follows :

A mixture of pulverulent ore, mixed with a suitable proportion of fluxing materials and reducing agents, is prepared, and from four to 5 tons of it are charged from a charging platform into the heated chamber to the depth of some 12 to 15 inches. But, before charging the mixture, some coke dust or anthracite powder is spread over the bottom and sides of the chamber to protect the silica lining of the same. The heat of the furnace is thereupon raised to a full welding heat, care being taken that the flame is as little oxidising as possible. The result is a powerful superficial action upon the mixture, or batch, causing simultaneous reduction of the ore and fusion of the earthy constituents. In the course of two hours a thick skin of malleable iron is formed all over the surface of the mixture, which, on being withdrawn by means of hooks, is consolidated and cleared of cinder under a hammer, and rolled out in the same heat into rough sheets of bars, to be cut up and finished in the refinery furnace or charcoal hearth. One skin being removed, the furnace is closed again, and in the course of an hour and a half another skin is formed, which, in its turn, is removed and shingled; and so on, until, after three or four removals, the furnace charge is nearly exhausted. A fresh charge is then added, and the same operation continued. Once every twelve hours the furnace should, however, be cleared entirely, and the furnace lining be repaired all round.

The shingled metal so produced forms an excellent melting material for the open-hearth or Siemens-Martin process; but if ores both rich and free from sulphur and phosphorus are used, together with roll and hammer scale, which forms an admirable admixture, I simplify the process still further in causing the fusion to take place in the reducing furnace.

The furnace having been charged with, say, five tons of batch, the heat is allowed to play on it for four or five hours, when about two tons of hematite pig iron are charged upon the surface by preference in a heated condition. The pig metal, on melting, constitutes a bath on the surface of the thick metallic skin previously formed, and gradually dissolves it on the surface while it is forming afresh below, and in the course of from three to four hours the whole of the materials charged are rendered fluid, consisting of a metallic bath with a small percentage of carbon, covered with a glassy slag containing about 15 per cent. only of metallic iron. The carbon of the bath is thereupon brought down to the desired point of only about 1 per cent. of carbon, and spiegeleisen or ferro-manganese is added, and the metal tapped in the usual manner. By these means the direct process of making cast steel is carried to a further limit than I have been able to accomplish before, and no difficulty has presented itself in carrying it into effect. The steel so produced is equal in quality to that produced by the open hearth process as now practised. If light scrap, such as iron and steel turnings or sheerings, are available, these may be mixed with advantage with the batch to increase the yield of metal.

These are, in short, the more recent improvements in the direct process of producing an iron and steel which I have been able to effect, and which I should have been glad to lay before the Iron and Steel Institute in a more complete form than I am able to do at the present time.

THE Health Committee of Glasgow, says the *Analyst*, seems to be going rather ahead of the sanitary boards of other cities, and we think very wisely so. They are carrying on continuous observations, at different stations in the city, on the variations in the composition of the air, and have expended a considerable sum of money in fitting up a laboratory in order to determine the variations which occur from time to time in the composition of the atmosphere itself, and the character of the floating particles which are present in it. The committee certainly deserves the utmost credit for taking a step so far in advance of any which has been taken by any ordinary public body.

THE REGULATION AND QUALITY OF THE SUPPLY OF FRESH AIR IN VENTILATION.*

From "The Architect."

In determining the quantity of air to be supplied to and removed from a building, and adjusting the corresponding capacity of the air-shafts, it is customary to assume standard temperatures of the air inside of the building, and of that outside which must be heated before distribution. The latter is taken as the average winter temperature. The height of the building and of the exhaust shaft being known, it is easy to calculate the approximate sizes of the air-ducts required for the supply and removal of a required quantity of air in a certain time say in cubic feet per minute. But the great range of temperature in our climate, often touching extremes as far as 50° Fah. apart within twenty-four hours, is a sufficient reason for not adopting any such rule, without taking extraordinary precautions. Supposing the flow of air in the shafts on a mild day to be represented by the number 25, the relative flow on a cold day would be 30. Economical considerations require that it should be just the reverse, since more fuel is required, first, to heat the extremely cold air; and second, to heat the increased quantity : which involves a waste amounting to 17 per cent. Then the natural ventilation of the building—that which takes place through its cellar, walls, windows, doors, and roof, which is not taken into account in a scheme of ventilation—is materially increased in cold weather, and a large amount of heat must be expended to meet it. But the greatest obstacle of all is the prevalence of high winds which accompany, or rather bring on, a low temperature.

The force which determines the movement of air in shafts is a very small quantity, being only the difference between the weights of a column of air as high as the exhaust shaft, having the temperature of the outflowing air, and of a similar column having the external temperature. This force is, in most cases, less than one-hundredth of that of a strong north-west wind.

To counteract the effects of temperature and wind, supposing that there is no force employed except that obtained by heating the foul air in the exhaust shaft, the most careful study and judgment will need to be used in locating the fresh air openings, proportioning and arranging the flues, and in regulating the exhaust. Valves in the inlet shafts are usually employed; and, since appliances to regulate the flow of air under varying conditions must be adopted, it is important that they should be few in number and simple in arrangement. The occupants will be fortunate if the ventilating apparatus of their building receives intelligent and faithful superintendence; and since the design is likely to be defective, and its working vexatious to the best-meaning manager, there is no sense in blaming him for relying upon his own judgment, and securing what seem to him the best possible results. Let there be but one inlet, with a valve to regulate the supply, and a pressure meter to show at a glance the volume admitted per minute; one heating chamber, with an arrangement for adjusting the temperature of the warmed air, an indispensable but never seen appliance; with a thermometer to show the temperature, and a valve in the only outlet shaft, operated from the same point as that in the inlet. With this arrangement only three simple operations are required to regulate the quantity of air and its temperature, and the results of all are immediately manifest. If a mistake is made, it can be instantly corrected, without waiting for complaints from the suffering occupants above, since the engineer sees exactly what he is doing. If he once finds that he can make any required change, his interest and gratification are increased, and he will take a pride in the certainty with which he can secure exactness.

The disposition of details in ventilating work must be directed by experience

* Paper read at the meeting of the Social Science Association at Saratoga, Sept., 6, 1877, by Mr. Frederic Tudor.

alone. The books on the subject, so far as they give us history and examples, are valuable; but even these are defective, since they seldom give well-observed facts and results. As to the purely theoretical works, they are of no practical value whatever. The formulas which they give are deduced from limited data, derived from the simplest cases. The higher mathematics cannot deal with quantities made up of a large number of variables; and a scheme of ventilation is entirely composed of varying conditions, variously connected and growing out of each other. Mathematical and physical research do give valuable data for use in ventilation, but they do not teach the art, nor do they throw much light upon the more complicated details which are the chief embarrassment of the work. The art of ventilation must be learned like any other art or trade, by practice combined with instruction; and expertness can be acquired by those only who are close observers, who have a certain intuitive judgment, and who have taken time to devote themselves to a thorough practical investigation of the subject.

Much care must be taken to avoid impairing the quality of the supplied fresh air in the process of warming it. The natural purity of the fresh air may be diminished by contact with red hot surfaces of iron, which will extract a portion of its oxygen, especially the oxygen which is united with hydrogen in the form of invisible vapor, whereby the degree of humidity is reduced, and hydrogen set free in the air; which will charge it with the unpleasant gases given off by dust and dirt which have been carried into the heating apparatus from the street, when heated to a high temperature, or by leakage of the products of combustion from the heating apparatus. The healthfulness of the air may be still further reduced by neglecting to add to the amount of its vapor while increasing its temperature. All these evils have been found to exist where furnaces of cast iron are used; and, without considering whether they might be owing to defects of construction, it has been hastily assumed that the material—cast iron—was the cause of them all. The partial investigations of MM. Deville and Troost, undertaken at the instance of Gen. Morin, have been accepted as finally condemnatory of cast iron as a material for stoves and furnaces. They, in reality, only prove that the apparatus with which they experimented was imperfect in construction, as might be expected in a stove of the cheapest possible character. The competition to sell merchandise, and to buy the cheapest offered, has resulted in the gradual elimination of all qualities of excellence in warming apparatus. As to the diffusion of the poisonous gas—carbonic oxide—through the imaginary pores of cast iron, it amounts to nothing, and can scarcely be detected with the most extreme care, even in air exposed to prolonged contact with the hot iron; but the free passage of this gas through the imperfect joints of the ordinary cheap furnace amounts to a great deal. And all the evils arising from over-heating will certainly be found when the heating surface has been so reduced as to make it necessary to keep it red-hot all the time. It is now generally thought that wrought iron is a more suitable material for furnaces than cast iron. This is true, but is no new discovery. The objection to it has always been the great cost of working it; and, in order to compete with cast iron furnaces of good quality, the makers have been compelled to omit the best features, and admit defects which make them in every way inferior to an old-fashioned, honestly-built, cast iron furnace. A good wrought iron furnace cannot be cheap; its cost should be certainly double that of a cast iron furnace of equal power. Since, then, the latter can be made to answer all requirements at a less cost, it would seem to be better adapted for general use.

But in any case good results cannot be obtained from cheap and inferior apparatus. The tendency to cheapen everything in the point where there is only an appearance of value, and only substance enough to enable the object to stand up until its sale is completed, will overpower and destroy the wrought iron furnace in its turn. It is now about to bring discredit upon steam apparatus, and it is extremely doubtful if there is a single case of heating by steam on a small scale which could not be better done by a good furnace at half the cost. For small dwellings and schools, where economy must be taken into account,

liberal expenditure for the best, but only the best, furnace work will be infinitely better than a cheap and half-complete steam apparatus, constructed at nearly twice the cost. Lest these ideas should seem to be retrograde, let it be borne in mind that it is not the best that is sought for, but the best for the least money, which is a very different thing from the cheapest. In other words, a superior article of an inferior class is to be preferred to a poor article of a higher class, and it generally costs much less.

There is another way in which the quality of the air is dependent upon the heating apparatus. It is the means whereby the relative humidity is kept up, or moisture added in proportion to the increased temperature of the air. All natural air contains among other constituents a portion of watery vapor. The amount is very variable, but increases rapidly from lower to higher temperatures. By agreement among chemists, the total amount which the atmosphere can hold at each degree of the thermometer is called saturation. At 32° Fahr., it is two grains, and at 70° it is 8 grains per cubic foot. The excess above saturation is always manifest in the forms of clouds, fog, dew, rain, or snow. The actual amount found at any time is compared with the amount required to saturate it at the same temperature, and is stated as so much per cent., and called the relative humidity. In the Northern States the average relative humidity is 70 per cent., in England 80 per cent. Now, if we take air at 30°, of 50 per cent. relative humidity, and heat it to 70° without adding moisture as Nature does, we shall have a warm air of only 12 per cent. humidity. It is sufficient to say that no observation of natural air has ever been made which showed so low a degree of humidity. As far as the writer knows, the lowest relative humidity is found in spring on the eastern coast of Massachussetts, during the prevalence of the so-called damp east winds. So far from being damp are they that they are the very driest known, showing sometimes as low as 30 per cent. They are, according to the writer's observation, always followed by epidemics of catarrhal troubles; are unspeakably disagreeable, and detested by everybody. North west winds in the early spring, when the earth is partially covered with melting snow, often show the same character, and are mistaken for east winds by unobserving people. The cause of their dryness is to be found in the absorption from the atmosphere of its vapor by the exceedingly cold water of the Arctic current east of New England. It seems paradoxical that water should absorb moisture from the air, but it takes place precisely as frost is deposited on a cold window-pane. A melting block of ice placed in a dish, and balanced on a pair of scales in a warm atmosphere, will shortly weigh down the beam by aid of the weight of the moisture or dew precipitated upon it. In the same way the west wind, already comparatively dry, yields up its water to the masses of melting snow. The instinctive dread with which these winds are regarded is corroborative proof of the close connection of the low rate of humidity with many diseases of the breathing organs, without ascertained facts showing them to be the cause. But since the characteristic of spring air is its dryness, and during that time such disorders are prevalent, it is safe to conclude that dryness is the cause, or at least an accomplice.

Such are the effects of a dry atmosphere at low temperatures, when the air although saturated holds but little water. What may be the effects of the dry air at the high temperature of our houses, which is relatively even dryer than the hated east wind, and which has five or six times the capacity for absorbing moisture from the lungs and skin which the colder natural air has? Perhaps the doctors can identify disorders, not febrile, but which could be explained by supposing the blood to have lost a portion of its water which in health is invariable in quantity. Would it be safe to hint that rheumatism and neuralgia might be some of these, and that there might be others, of a febrile nature with a specific cause, which owed their origin to the inability of the deranged blood to repair waste and fortify the system against attacks which healthy people in other climates are not subject to?

It was said before that the relative humidity was variable even in natural air. In fact, it varies from hour to hour,

but in pleasant climates it seldom falls below 65 per cent., and the more limited the range the more agreeable the climate. Increased ventilation means diminished humidity, since there are many unrecognized sources of moisture in every house, and if the air is frequently changed, of course these are less able to keep up the supply. In most well-ventilated furnace-heated houses the humidity will not rise above 30 per cent. in cold weather, while in buildings heated by steam or hot water, on the indirect system, the amount will generally be below 20 per cent., sometimes at 15.

It is important to avoid all schemes of ventilation which provide for the admission of unwarmed air through windows or wall-openings. It is next to impossible to so arrange the distribution as to avoid draughts, which will certainly lead to the permanent closing and neglect of all such openings. The more draughts there are, or the more rapid the movement of air in schoolrooms and halls, the higher should be the temperature and the relative humidity. All the sting is taken out of a draught if it is warm and unable to chill the body by absorbing the insensible perspiration too rapidly, as it cannot do if near the point of saturation. But if the distribution is well managed it is safe to introduce air as low as 65°. Any plan of ventilation which involves a multiplicity and complication of details will be certain to fail, either from ignorance of its construction and purpose, or from neglect and indifference to results. Finally, the requisites of a successful scheme are—the reduction of the number of parts, control of the movement of air at one point by one operation, regulation of temperature without interfering with the quantity of air supply, accessibility to all parts so that they can be cleaned or repaired without trouble, all the unavoidable mechanical movements to be as simple and self-evident as they can be made, and suitable apparatus for the evaporation of water to be included. It is safest that the entire plan of the heating and ventilating should be decided upon and incorporated into the general plans of a building before the first step is taken towards its construction.

THE PORTABLE ENGINE OF THE FUTURE.

From "The Engineer."

We have good reason to believe that the demand for portable engines to be used in Great Britain, which has been becoming smaller and smaller for some time past, will soon cease almost altogether. The only engines of the kind which will be built in a not distant future will be sold in foreign markets or purchased for home use solely by builders and contractors. This statement will hardly take any of the great agricultural engineering firms by surprise, however startling it may appear to others. But no alarm need be felt; an important branch of trade will not be cut off. The place of the portable engine will be filled by a kindred machine, in the production of which profits may be made and a large business done. In order that we may understand what this machine will be—what, in a word, is to take the place of the portable engine—we must consider the nature of the circumstances which have led to the change. In Great Britain portable engines are bought—putting contractors on one side—by two distinct classes. One of these consists of men farming large tracts of land and possessing two or more homesteads, often some miles apart. These agriculturists use the portable engine almost exclusively for thrashing; and the engine and machine have to be moved as occasion requires from farm to farm. The removal will require, on an average, the services of six horses—four to draw the engine, and two to draw the thrashing machine. The horses are taken for this purpose at a time when every hour is of importance to the farmer who wishes to get his land ploughed, or his roots led home; and this the owner of the engine, machine, and horses regards as a very serious evil. The second class of British

purchasers consists of men who either hold no land, or very little. They invest some hundreds of pounds in the purchase of one or more sets of thrashing machinery, and they make a livelihood by hiring them out to farmers who do not grow corn enough to give employment for more than a few days in the year to a thrashing machine. The rule is that each hirer of the set shall send his horses for it to the place where it was last employed. If the tiller of hundreds of acres grumbles at the loss of the services of six horses for a day or two, how much more inconvenient must the loss of his teams prove to his poorer neighbor? The difficulty of the case is of course augmented when, as sometimes happens, the small farmer has not got six horses to send for the engine and thrashing machine. So long as there was no help for this state of affairs, the farmer had perforce to submit. But he has not been blind to the fact that certain owners of the machinery which he wanted did not use portable but traction engines; and for a very small extra hire —sometimes without any additional cost whatever—engine and machine came together to his stackyard, thrashed as much corn as he wanted, and went away without at all interrupting his field work. The result is that he who possesses a traction engine always has the pick of the market for his thrashing machinery; and it has already been discovered that it will not pay to purchase portable engines to be hired out, while a neat little income can be realized if the engine will propel itself and a thrashing machine from farm to farm. The holder of great tracts of tillage land finds precisely the same benefit result from the substitution of the traction for the portable engine, and it is thus day by day becoming more evident that those who wish to build agricultural engines for sale in this country must give them powers of self-propulsion.

Are we then to believe that the traction engine will take the place of the portable engine? The answer to this question will depend altogether on certain conditions; and bearing this in mind, we may say that it is not unlikely that a demand will exist for two classes of engines. The large landholder will probably prefer a portable engine fitted with self-propelling gear, because such an engine will be lighter, simpler, and cheaper than traction engines. But those who purchase engines that they may hire them out will prefer traction engines whenever it is likely that they can get employment for them in hauling coal, corn, manure, &c., on the high roads, because in such cases the engines need hardly be idle at all throughout the year; whereas engines fitted only for thrashing are worked for but a couple of months or so at the most, and lie by for the rest of the time. It is quite impossible to say for which class of engines the greatest demand will spring up, but it is not difficult to define the conditions which will tend to limit the demand for traction engines proper. We may assume, for the moment, that the state of the law will exert no material influence one way or the other, and we must look therefore to the machine itself for the causes which will affect its popularity. Comparing the traction engine with the portable engine, it is obvious that the former is, for a given power, much heavier than the latter. This increase of weight is caused not only by the presence of the gearing required to drive the road wheels, but by the augmentation in strength which must be carried out right through. A traction engine ought to be stronger in all its parts than a portable engine. If it be not stronger, it soon knocks itself to pieces on the roads, as the traveling strains, as we may term them, to which it is exposed, are much more severe than those which the portable engine has to withstand. It would be easy to explain why this is so; but we fancy our readers will be content to accept as proved what they must very well know to be true. Now augmentation of weight in engines intended to travel over ordinary parish roads is very objectionable, because of the multitude of small bridges and culverts which are far too weak to carry heavy loads. We could cite instances wherein ploughing engines have had to make a round of six or seven miles to go from one farm to another not more than a mile off, in order to get to a bridge strong enough to carry them. It may be said that the bridges ought to be made strong enough. We shall pronounce no opinion on this point; but

whether they ought to be strengthened or not, it is at least certain that there are large districts of country in which at present the use of any engine weighing more than six tons is almost impossible, because of the weakness of certain parish road bridges spanning small streams. Another factor tending to limit the demand for traction engines is found in the cost of repairs, which it is not too much to say never comes to a sum less than twice as much as would suffice to keep a portable engine in excellent order, while it is often five or six times as great.

If, now, we compare the self-propelling engine with the traction and the portable engine, it will be seen that several points may be urged in its favor. In its simplest form the self-propelling engine is an ordinary portable, a little strengthened in a few places. On the crank shaft is fitted a chain pinion. The hind axle revolves in bearings at the back of the fire box, and is driven by a chain wheel keyed on it. The road wheels are caused to revolve by shifting pins in the usual way, which pins can be removed at either side when a sharp corner has to be turned. At the back of the fire-box is hung a kind of removable tray or footplate for the driver, which provides room for about 1 cwt. of coal, any additional quantity required being conveyed in bags on the thrashing machine. Water is carried in a tank under the barrel of the boiler. Engines of this type were built several years ago by Messrs. Barrett, Exall, and Andrews, of Reading, and possibly by other firms, and answered very well. At the time, however, no sufficient demand had sprung up for self-propelling engines, and but few were made. Of such engines, it may be urged that they can be much lighter than a traction engine. So much lighter, indeed, that any bridge which will carry a portable engine may be trusted to sustain one. They will also be cheaper as regards first cost and repairs than a traction engine. But on the other hand, it must not be forgotten that their use will be practically limited to the performance of the ordinary duties of a portable engine, and they will be unfit to draw heavy loads. The intending purchaser will no doubt bear these facts in mind, and decide which class of engine will best suit his purpose; but agricultural engineers may feel certain that for both kinds of engine a large demand will spring up ere long. Out of the manufacture of which class of engine the greatest profit may be made we cannot pretend to say. But it would appear that before laying himself out for the production of either the one or the other, the engineer should study the nature of the district in which his customers live, and shape his plans accordingly.

It is not improbable that a good market might be found for a fourth type, namely, a very light traction engine, which would be competent to convey moderate loads at a speed a little greater than that of horses, say, four to four and a-half miles per hour. In order that such an engine may be kept as light as possible, it should have but one speed, and either carry steam of, say, 80 lbs. to 90 lbs. pressure, or else have the cylinder made a little larger than usual, in order that sufficient power may be available when a bit of steep hill has to be surmounted. It will be essential to the success of such an engine that it is carried, as regards the driving wheels, on springs. These need not be very flexible, but their use would permit the weight of the boiler, and most of the moving parts, to be kept down to those of the ordinary portable engine. With the demand no doubt engines of this type will spring up. In a word, the traction engine and its congeners is coming once more into prominence, and so much experience has been acquired in the practical use of steam on common roads within the last twenty years, that a very successful result may be anticipated. However, some men will make mistakes, and we shall not be surprised if certain very singular examples of the self-moving agricultural engine are found in the Agricultural Hall at Islington next December.

THE FRENCH IRON TRADE.—The French iron trade is suffering, to some extent, from the uncertainties attending the French political situation. Prices have, however, been pretty well maintained at Paris, merchants' iron having made £7 4s. per ton. Pig is generally quoted at £2 12s. per ton in the Meurthe-et-Moselle.

HISTORICAL SKETCH OF EXACT RECTILINEAR MOTION.

By G. BRUCE HALSTED, Fellow of Johns Hopkins University.

Written for VAN NOSTRAND'S MAGAZINE.

THERE has appeared in Macmillan's "Nature" series a little book with the queer title, "How to draw a straight line." Now, though one might not suspect it, this little book contains the elementary principles of a subject of highest scientific importance, which should be of especial interest in America, the land of practical applications. Its author is A. B. Kempe, the same who in 1875 presented a paper to the Royal Society, "On a general Method of producing exact Rectilinear Motion by Linkwork," which was reprinted in Van Nostrand's Magazine. This article does not seem to have attracted here the attention it deserves. The desire to awaken a little more interest in the book has moved me, as much as the intrinsic merits of the subject, to prepare a short historical sketch of a chapter of progress of which this little book is the last and most elementary outcome, a chapter which seems to furnish a very beautiful example of how the torch of science is passed from hand to hand, from land to land.

It is well known that the so-called Parallel Motion used in steam-engines was invented by James Watt, and in the specification of a patent granted in 1784 is described by him as "directing the piston rods . . . . so as to move in perpendicular or other straight or right lines."

But the motion really produced is a sort of figure eight, no part of which is straight.

Now, it seems astonishing that in spite of the geometrical genius of the Greeks before the Christian era, and the eighteen centuries of time after it, no method had been discovered of describing a straight line, that simplest of all lines. Yet such was the case, and even when it assumed the highest practical importance in the steam engine, men were content with imperfect, approximate solutions. And well they might be, for as Prof. Sylvester has said, "the more he reflects on the problem to be solved, and the nature of the solution (essentially a process of transformation operating on polar coordinates), the more he wonders that it was ever found out, and can see no reason why it should have been discovered for a hundred years to come." It was first obtained in 1864, eighty years after Watt's patent, by M. Peaucellier, then an officer of engineers in the French army.

He *first* solved in a manner, absolutely rigorous, the problem of converting circular into rectilinear motion. He announced it in general terms in the form of a question in the "Nouvelles Annales de Mathématiques," 1864.

Still he did not seem to appreciate the importance of what he had done, nor did it catch the attention of any one prepared to see its value; so it fell into oblivion for six years.

Yet there was at least one man particularly fitted to be vividly struck by it and see its importance.

This was the great Dr. Tchebicheff of Saint-Petersburg whose mathematical genius has been likened to that of Pascal. He had long occupied himself in attempting to prove the *impossibility* of the exact conversion of circular into rectilinear motion.

It would be interesting to investigate how it came about that in 1870, only six years after the first discovery, this wonderful conversion was rediscovered just in the right place, that is, in Saint-Petersburg, by one of Tchebicheff's students named Lipkine. His professor obtained for this fortunate youth a substantial reward from the Russian government, and this has since stirred up that most conservative body, the Institute of France, to confer its great mechanical prize, the "Prix Montyon," on Peaucellier, who gave in 1873 a detailed exposition of his discovery in the same journal which had published his first intimation nine years before. Meanwhile Lipkine had presented the theory and description of his apparatus to the Académie de Saint-Petersburg in 1871 and exhibited a model of it at the Vienna Exposition in 1873.

Some months after, Dr. Tchebicheff visited England, and there Prof. Sylvester happened to ask him about the progress of his proof of the impossibility of the exact conversion of circular into rectilinear motion.

Tchebicheff answered that, far from being impossible, it had actually been accomplished, first in France and subsequently by a student in his own class. He then left with Sylvester a drawing of the cell and mounting, consisting together of seven links. He had proved at least, he said, that it could not be accomplished with less than seven links.

Shortly after this memorable interview, Dr. Garcia the eminent musician and inventor of the laryngoscope, happened to visit Prof. Sylvester, and being shown the drawing, sent to the Professor next morning a model constructed with pieces of wood fastened together with nails as pivots, which, rough as it was, worked admirably, and drew forth the most lively expressions of admiration from some of the most distinguished members of the Philosophical Club of the Royal Society. Soon after, Prof. Sylvester exhibited the same model in the hall of the Athenæum Club to his brilliant friend, Sir Wm. Thomson, "who nursed it as if it had been his own child, and when a motion was made to relieve him of it, replied, 'No! I have not had nearly enough of it—it is the most beautiful thing I have ever seen in my life.'"

Prof. Sylvester's appreciation carried itself from admiration to accomplishment. He changed what seemed an isolated fact into a grand theory. He proved that every possible algebraic curve may be described by linkwork. This proposition as far as concerns curves of the first nine genera, and also for curves of the first six orders, or for any order where the degree of one of the variables in the representing equation is five or less, he demonstrated by a direct method. In using this method he found it necessary to prove that a general equation of the fifth degree can always be reduced to a trinomial form by *real* transformations, which, by Tchirnhausen's (the only method hitherto applied), as often as not is incapable of being done. This was a most remarkable discovery, which renders general and always valid M. Hermite's celebrated memoir on equations of the fifth degree.

Prof. Sylvester was invited to deliver a lecture on this new theory at the Royal Institute, which he did, Friday, Jan. 23, 1874. There he stated that we are able to bring about any mathematical relation that might be desired between the distances of two of the poles of a linkage from a third, and are thus potentially in possession of a universal calculating machine. He exhibited and worked a cube-root extracting machine constructed on this principle, and claimed to have given the first really practical solution of the famous problem proposed by the ancients of the duplicated or multiplication of the cube. He stated that Dr. Tchebicheff had informed him that he had succeeded in proving the non-existence of a five-bar link-work capable of producing a perfect rectilinear motion, but there might be other seven-bar linkworks capable of solving the problem.

Fired by generous enthusiasm, and working on this hint, two young Englishmen, graduates of Cambridge, took up the subject. The one, Henry Hart of Woolwich, among other theorems of great interest, made the astonishing discovery that in spite of Tchebicheff's declaration of its demonstrated impossibility, he could construct a linkwork of only five bars which would exactly convert circular into rectilinear motion. This has since been proved beyond all doubt by such men as Cayley, Roberts, &c., to be absolutely the smallest possible number.

The other young man is A. B. Kempe, to whose article in VAN NOSTRAND'S MAGAZINE and subsequent little book we so much desire to call the attention of Americans. What he has done may best be learned from his own writings. Our earnest wish is that this science of linkages, which has traveled thus from France through Russia to England, growing constantly, may be taken up in this country, and that the American genius for application may add to the beauty of theory the beauty of practice. It seems to have illimitable possibilities in regard to all constructions in machinery.

We add a complete bibliography of

the subject, which here, without diagrams, it was impossible even adequately to adumbrate.

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18. The substance of (17) was previously given as a Lecture at South Kensington in connection with the Loan Collection of Scientific Apparatus. See "Nature," Vol. 16. No. 395, May 24, 1877, pp. 65–7, No. 396, May 31, pp. 86–89, No. 398, June 14, pp. 125–127, No. 399 June 21, pp. 145–146.

THE RESTORATION OF THE ANCIENT SYSTEM OF TANK IRRIGATION IN CEYLON.

By R. ABBAY.

From "Nature."

A WORK apparently pregnant with the largest and most beneficent results to the native population of Ceylon is in process of being carried out by the Colonial Government of that island. More than a thousand years ago a system of irrigation, the most complete and remarkable that the world has ever seen, was in successful operation in the Low Country, and the object which the Government has in view is to restore to something like its pristine fertility a large proportion of the immense tracts of land—many hundreds of thousands of acres in extent—that for want of water have fallen into a condition of the most utter sterility. Sir Emerson Tennant, writing twenty years ago on this subject, says, "The difficulties attendant on any attempt to bring back cultivation

by the repair of the tanks are too apparent to escape notice. The system to be restored was the growth of 1,000 years of freedom, which a brief interval of anarchy sufficed to destroy, and it would require the lapse of long periods to reproduce the population and recreate the wealth in cattle and manual labor essential to realise again the argicultural prosperity which prevailed under the Singhalese dynasties. But the experiment is worthy of the beneficent rule of the British Crown under whose auspices the ancient organization may be restored amongst the native Singhalese."

The origin of the system of irrigation spoken of dates as far back as the year 504 B.C., when, according to the Singhalese Chronicle, Mahawanso, the first tank was built in the neighborhood of his new capital, Anuradhapoora, by Panduwasa, the second of the Hindu Kings. This was succeeded about seventy years later by two others formed in the same neighborhood. In the year 459 A.D., the Kalawewe Tank, the largest of all, was completed. The retaining bund of this immense sheet of water is twelve miles long, and the circumference of the lake which it formed was no less than forty miles, the water being backed up for a distance of fifteen miles and conducted from the tank by means of a conduit sixty miles in length to the capital. Sir Emerson Tennant in describing these remarkable reservoirs, says, "Excepting the exaggerated dimensions of Lake Mœris in Central Egypt, which is not an artificial lake, and the mysterious basin of Al Aram in Arabia, no similar constructions formed by any race whether ancient or modern exceed in colossal magnitude the stupendous tanks of Ceylon." The same author estimates that at the time of its greatest prosperity the island contained a population of from fifteen to twenty millions, nearly all of whom must have derived their means of sustenance from irrigated lands. At the present moment, after all the care bestowed through three-quarters of a century by a paternal government, the population only amounts to 2,400,000, whilst even for this a large proportion of the food—6,000,000 bushels of rice annually among other things—has to be imported from India, and the population itself must be considered to have been somewhat unnaturally increased during the last fifty years by the stimulus of European enterprise. The mass of the people too have changed their place of residence from the interior to the neighborhood of the sea-coast, where trading and fishing instead of rice-cultivation furnish them a livelihood. The vast areas which formerly under the magic influence of a sufficient supply of water and a hot sun, produced their two or three crops of rice in a year are now absolutely deserted, frequently not a single inhabitant surviving where once a thousand found ample means of subsistence. The city of Anaradhapoora, if its ruins afford us any means of estimating its magnitude, must have covered an immense area—no less than from thirty to forty square miles, and the population living on the spot and drawing its supplies of food from the immediate neighborhood must have been correspondingly immense. Now it is a mere village in the midst of vast heaps of ruins.

One of the most gigantic of these early irrigation works is supposed to have been originated by Maha Sen about the year 275 A.D., and, having been enlarged by Prakrama, Bahu I., who reigned in 1153, to have received from him the name of "The Sea of Prakrama." It consisted of a series of lakes formed by an embankment twenty-four miles in length and from forty to ninety feet high, by which the water of a large river and many considerable streams was hemmed in along the base of a range of hills and so forced into the valleys that a series of lagoons or lakes was formed extending for the above-mentioned distance, and frequently several miles in width. A canal five miles in length conducted the waters of "the sea" to the Minery Lake, another of the works of Maha Sen, to be mentioned presently, and a further canal from Minery led the waters to the neighborhood of Trincomalie, in all a distance of fifty-seven miles. When it is remembered how sudden and torrential the rains are in a country like Ceylon —the writer has known 18 inches of rainfall in forty-eight hours over a very large extent of country, and at one spot as much as 18.9 inches in twenty-four hours,—we cannot too much admire the vastness of such a work and the skill

which enabled the native engineers to use the natural features of the country in such a manner that, for a distance of twenty-four miles, a single embankment sufficed not only to hem in the water for purposes of irrigation but also to provide a water-way for the transport of produce and merchandise. Along the whole course of this embankment and canal and wherever its tributaries carried the life-giving water there would be without doubt a teeming population; for irrigable land in Ceylon is capable of supporting, according to official calculation, 1,000 persons to the square mile. In 1855 there was not a single inhabited village, although a few patches of land were occasionally cultivated by people from a distance. The contrast between the remote past and the present condition of this half of the island is a painful one to contemplate, but it is to be hoped that the Colonial Government will never stay its hand until all the useful works of ancient times have been restored and improved—but this will be a work of centuries.

Long before the Christian era the main ambition of the kings of Ceylon appears to have manifested itself in the formation of tanks, and many kings are mentioned in the Mahawanso who, "for the benefit of the country," and "out of compassion of living creatures," built a dozen or more of these splendid, but absolutely necessary, irrigation works. The Minery tank, some twenty miles in circumference, and irrigating an enormous area of fertile land now entirely barren, owed its origin, along with sixteen others, to Maha Sen, who reigned about the year 250 A.D. It is now merely a swamp, resorted to by enormous numbers of wild fowl. Up to the twelfth and thirteenth centuries Ceylon produced her own supplies of food, but in the fourteenth it appears that the island was obliged to import a portion of it from India. In 1301, it is related that there were 1,470,000 villages in Ceylon. In 1410, as many as 1,540,000, the term village implying hamlet, or even a single house where there are people resident. Of the vast majority of these, if they ever really existed, not a vestige is left except the ruined tanks, which show unmistakably where the foci of population formerly were. This was shortly after the conquest of the island by the Malabars, who are believed not to have actually destroyed the fabric of the embankments, but by their system of government to have disorganised the village communities to such an extent that the works connected with the tanks fell into disrepair through neglect, the land became imperfectly irrigated, and the population gradually died out. That this process was a perfectly natural one seems evident from the fact that the tanks do not show any traces of wilful damage, and also from the consideration of the almost innumerable evils resulting in death, of which a scarcity of water in a tropical country like Ceylon is productive. Indeed one of the most frightful diseases that have ever scourged the human race is believed to have been developed in these very localities chiefly through the want of proper food, caused by the absence of a system of irrigation. It is believed, too, and there is strong evidence, based on experience, for the belief that the disease entirely disappears wherever irrigation is restored. It will naturally be asked, "If the advantages of a plentiful supply of water are so enormous, why have not the tanks been restored before this, and what hinders their immediate restoration at the present time?" The reply is, that the creation of this magnificent system of irrigation was not the work of a decade, or even of a century, but of a thousand years of successful national development, and that therefore the restoration of it must be also a work of time.

The object of this paper is to draw attention to the fact that the experiment of restoration is at the present moment in process of being tried, and bids fair, after the lapse of half a century or so, to alter entirely the character of the island. The most remarkable success has already attended the efforts to afford irrigation facilities to the Singhalese on the East Coast. Where but a few years ago the natives where half-starved, and the land apparently in a hopeless condition, the re-introduction of irrigation through the assistance of the Government has transformed not only the people, but the country, as if by magic. Rice-fields, palms, and other fruit-trees abound, and the population is increasing at a rapid rate. Of this particular district the

present Governor of Ceylon (Sir William Gregory) reported some four years ago to the Legislative Council of the island in the following terms :—"In the month of April I visited the rice-growing regions of the Eastern Province, which are the creation of the irrigation works carried out by the Government. I never before saw such an unbroken sheet of grain. Save where some isolated trees, part of a recent forest broke the view, the eye wandered over some 20,000 acres of green paddy. I saw, wherever I went, a sleek, vigorous, well-fed, and thoroughly healthy population. Up to 1864 the lands under cultivation in this province were 54,000 acres, the chief impetus to the irrigation scheme having been given in 1857. In 1871 the lands in cultivation were 77,000 acres. The Crown lands to be additionally reclaimed under works already completed or in course of completion, amount to 15,900 acres, equal to the support of 23,850 persons." Again, speaking in the same report on the subject of the Great Tank already mentioned, he says: "I am most anxious to put the full strength of the department at work in restoring irrigation to Nuwara Kalawia. This magnificent district has the strongest claims upon us. It was once the granary of the island. It is now utterly neglected. It has a population of 60,000 persons and over 1,600 villages, which have each of them their tank. There are at least 1,700 of these tanks, and I am credibly informed not one of them has a sluice in order. I trust that a few years hence the population may present the same vigorous and thriving appearance as the population of the Eastern Province, and from the same causes—namely, good and plentiful food." Of this same district a gentlemen of very great experience told the writer that in traveling through it many years ago he came to a village where, of the thirty inhabitants, only one of them was able to carry water, all the others having been stricken down by hunger or disease. This destitution was caused by the failure of three successive rice-crops, and was not specially exceptional, but fairly representative of what takes place frequently in the district. If we compare the scenes of plenty and contentment as they exist in the Eastern Province at the present moment with what meets us in the Wanni, or in any of the northern districts, where tanks have not been extensively repaired, the contrast is most striking. We find an almost depopulated country, with here and there a wretched village peopled by a few miserable and more than half-starved inhabitants, who, in times of scarcity, which are not infrequent, are obliged to live on roots and wild herbs, who are periodically decimated by a frightful disease, yet who seem bound to the spot where they were born, and prefer to die there rather than move away to a more fertile and healthy district. It is, indeed, this disinclination which possesses the agricultural Singhalese to move more than a day's journey from his home that presents the greatest of all difficulties to the scheme for the restoration of the tanks. It is on this account that the process of restoration is always in advance of the supply of natives to take up the new land, unless the works happen to be in the immediate neighborhood of population. The only plan, therefore, that has proved really successful under present conditions is to restore the tanks in the vicinity of villages, and induce the population to creep slowly onwards step by step, cultivating the more fertile pieces of ground as it advances, until the depopulated districts shall have been partially reclaimed, when the completion of the work will be a matter of comparative ease. Two typical instances of this mode of procedure have been mentioned to me by an official high in the Government service, as showing the effect of a well-regulated expenditure of labor and money in restoring irrigation works. In the year 1854 Mr. Bailey, whose name will ever be associated with this scheme for benefiting the natives, spent less than £100 on a canal some miles to the north of Matalé, a country town a few miles north of Kandy. The village thus supplied with water had previously dwindled away until only three houses were left, the rice-fields were deserted, and the famine-stricken inhabitants declared that they would die where their fathers had lived and died rather than migrate to a part of the country that was unknown to them. Ten years after the improvement was made the spot had become a little oasis in the desert; nearly two hundred

acres of rice were under cultivation, yielding about thirty bushels per acre, and supporting a population of several hundreds.* Almost in the same neighborhood a sum of between £200 and £300 was spent on an old canal fifteen miles in length by the same zealous Government official already mentioned. Many hundreds of acres were brought under cultivation, and in ten years' time, instead of a starved and fever-stricken population of 150 inhabitants, no less than 500 able-bodied men were on the list as liable to the road-tax. The changes in these, as in other instances, took place as if by magic, yet the means employed in effecting them were of the most limited and simple nature. The secret of the success lay in the fact that a famishing and disease-smitten population was within a few miles of the spot, and the remnants of ancient engineering skill were ready at hand to guide the laborers on to certain success. Since the above tentative experiments were made, very great changes for the better have taken place in the condition of the agricultural part of the native population. The carrying out of the scheme for the restoration of irrigation works is recognized as one of the chief duties of the Colonial Government, and there is little danger that, after the real success which has attended it so far, any future Government will allow it to be interrupted. The policy of the Colonial authorities may be summed up in the pregnant words of Sir Wm. Gregory's address to the Legislative Council in 1876:

"I consider that at least 100 tanks should be supplied with sluices, and properly repaired each year; and I have asked the Secretary of State to furnish me with an additional number of well-trained officers, by whom these works will be carried on with vigor. There is no boon which the Government can confer on the villagers more legitimately than this. It is a reward for their own exertions, and I am confident that each year, as it becomes better understood, it will be more appreciated, and that it will be recognised everywhere that the Government have no other object in it than to increase the comfort and resources of the people." It will appear, from what has been quoted, that the tanks are not repaired free of cost and then handed over gratuitously to the villagers, but the natives are required to give a certain amount of labor in restoring the tanks, and also to pay a small rent or tax on the land cultivated, so that, whilst the native cultivator is the chief gainer by the undertaking, the Government is no loser. If there could have been a doubt as to the wisdom of the Tank Restoration scheme, the experience of the last three years must have dispelled it and proved how absolutely necessary a system of irrigation is to the welfare of the natives. In the address above quoted, whilst speaking of the cholera and other diseases which had visited several of the provinces, the Governor says:—"It is remarkable that the inhabitants of the Eastern Province enjoyed perfect immunity from epidemics of all kinds. It is an interesting question, on which I do not give an opinion, whether this general immunity from disease in the Eastern Province is due to the abundant supply of food throughout the populous part of it, the result of irrigation works." At the same time he speaks of the restoration of two of the large tanks as complete. One of these will irrigate 23,000 acres, equal to supporting a population of 35,000 persons; the other will bring large tracts of magnificent land into cultivation, and dissipate the unhealthiness of the district which has hitherto prevented settlement.

To look back over the early history of the attempts under Sir Henry Ward to restore the above system of irrigation, is like reading the accounts of the commencement of a successful campaign. The difficulties encountered were sufficient to discourage even enthusiastic philanthropists, chief amongst them being the utter disorganisation of the village communities through the abolition of compulsory labor and the rooted dislike of the natives to migrate from one spot to another. For the recent part of the evil caused by this disorganisation the British Government was alone to blame, for in abolishing Rajekaria they abolished the right of compelling villagers to keep their tanks and watercourses in repair. By doing this they practically placed the distribution of the

* Irrigated rice-lands in the low country will support a population at about the rate of 1,000 persons to the square mile.

most valuable property of which the natives were possessed in the hands of the strongest, and consequently the most unscrupulous inhabitants of each district. In a dry season, when there was barely sufficient water to irrigate the fields along the course of a canal, those who were nearest to the source of supply would probably get more than their share, whilst those who were furthest from it and had an equal claim on it might get none; but, generally, the strongest party would get the advantage, to the ruin of the weaker. Dams would be built at various points along the course of the stream by one party, and as quickly destroyed by another. Interminable feuds were the results, and appeals to the courts of law, which, not being guided by native customs, only made matters worse. The canal, too, which ought to have been kept in proper repair by the united efforts of all who benefited by it, was allowed to fall year by year into a more ruinous condition, after compulsory assistance had been abolished, the residents on the upper portion of it refusing to aid those on the lower to repair the breaches made by the annual floods. Consequently the work that was done was ill done, and only of a temporary character. Soon it became beyond the power of isolated communities to effect the necessary repairs; the lands fell out of cultivation, and the population, after a long struggle with their neighbors, either died out or sought a living elsewhere. The early legislation in 1856 was based on a revival of the native customs and a compulsory distribution of the necessary work among the different villages, a majority of two-thirds of the inhabitants being enabled to place the lands under the Irrigation Ordinance, and to compel the assistance of all who benefited by the supply of water. The scheme resulted in complete success. It met the great want of the natives, and the interminable disputes about boundaries and rights of water, which was as much property to the natives as the land itself, soon ceased. The Government claimed its own and sold large portions of it by auction at a very reasonable rate, the upset price being generally £1 per acre, the land continuing to be chargeable with a yearly tithe to the Government of from 3*s.* to 4*s.* per acre. In special cases the Government granted even easier terms in order to induce the natives to settle in particular localities. Newly-purchased land was allowed to be free from tithes for four years, and the purchase-money was spread over an equal period from the time of sale. The pecuniary result was most gratifying to the Government, and the benefit conferred on the natives inestimable.

A few words will be sufficient to describe the character of the cultivation which this system of irrigation is intended to promote. A crop of rice, or paddy, as the undressed grain is called, requires about ninety days to come to perfection, and during this time it must be supplied with about thirty inches in depth of water, or a little over 4,000 cubic yards to the acre. The first and second watering of the paddy takes place within a fortnight of the sowing of the seed, and the water is only allowed to remain on the land for a short time. The three subsequent waterings take place about the twentieth, the fortieth, and the sixtieth days after sowing, from eight to ten inches of water being used each time, and the water is allowed to remain on the land until it has evaporated. This system, though more or less modified according to the climate and the supply of water, is fairly representative of rice-cultivation in the low-lands of Ceylon. The official estimate of the produce is about thirty bushels per acre. It is probable that exactly the same system existed in the very earliest times, and that the Singhalese engineers were able to regulate the flow of water through the tank sluices just as they wished. It certainly seems unreasonable to suppose that the men who could design such a vast irrigation system with no better means of levelling than that of leading water by actual experiment from one point to another, should fail in minor matters such as sluice-gates. Yet the writer believes that nothing is known as to the manner in which the flow of water was regulated. It is true that in some of the sluices a square masonry well is found leading upwards from the sluice soon after it has entered the embankment from the tank, but there is nothing left to show how it was used. Captain Sim, R. E., some years ago suggested that it

was intended to break the force of the water rushing in flood-time towards the sluice, and reduce the velocity of the water in the sluice to that due to the pressure in the well only. I am however inclined to think that a frame of wood, somewhat in the shape of a box strongly braced together, was fitted into the well, so that it could rise and fall readily under the influence of the water in the tank, and that by placing weights on the top, the frame might be forced down so as to cut off either partially or wholly the water issuing through the sluice. Wherever rocky foundations could be found for a dam, or a ledge of rocks for a spill-water, the native engineers, as if distrusting artificial constructions, would be sure to utilise them. In some cases, where it was possible to include masses of rock in the embankment, the sluices themselves would be cut out of the solid gneiss and the work thereby rendered as indestructible as the rock itself.

It will no doubt be somewhat surprising to persons who are only acquainted with the system of rotation of crops in vogue in Europe, that these rice-lands can be made to produce year by year for hundreds of years consecutively, one or two crops of grain annually without the land becoming exhausted or requiring to be continually renovated by manure. The explanation, however, seems to be that sufficient vegetable matter is carried down from the hills, partly in solution and partly in suspension, in water to supply all the waste produced by the continuous cropping. Those who have visited the richest alluvial valleys of California and Australia will no doubt have been struck by the fact that the most fertile soil is always found where the alluvium has been deposited in extremely fine particles, and in water practically at rest, conditions which obtain in the paddy fields of Ceylon, and must have obtained formerly on the Hunter River in New South Wales, and in the valleys opening on the Bay of San Francisco.

I cannot better conclude this paper than with an extract from a minute by Sir Henry Ward, after a tour of inspection in 1859:

"The village of Samantorre is a very fine one, and stands on the borders of the richest plain in Ceylon, containing, as it does, nearly 15,000 acres of paddy. Mr. Birch and Mr. Cumming informed me that the scene of joy and excitement exhibited by the whole population when the water first came down from the Ericammam, in July, 1858, and saved a magnificent crop from destruction by drought, was one of the most striking things ever witnessed. Hundreds of people had collected at Samantorre as soon as they knew that the sluices were to be opened; and when the water was actually seen advancing down the bed of the dried-up river, the shouts, the firing of guns, the screams of the women, the darting off of messengers bearing the news in every direction, made a deep impression on all who saw it. They felt that a great work had been done, a great benefit conferred. But I feel also that under British rule this benefit ought to have been conferred thirty years ago upon a people so capable of appreciating it.

Indeed, knowing what I now know of the history of the Eastern Province, I hold that what the Government is doing in 1859 is simply the payment of a debt incurred by our rash interference with a people of whose habits and wants we knew nothing. This error is now in part repaired. 44,000 acres of land are already under paddy cultivation, and I see reason to believe that the amount will be not less than 60,000 acres in 1861, when the irrigation works have obtained their full development. But this will require constant attention on the part of the Government and of its local representative. The maintenance of the *system* must never be lost sight of, and should unforeseen demands for assistance arise they must be met liberally and promptly." The words of so successful a governor have not been forgotten. The present governor, Sir William Gregory, has devoted all his energies to the carrying out of what was so well begun. The survey and engineering staff of the colony has been considerably increased, and the restoration of nearly the whole of the ancient irrigation works, besides the creation of new ones, may now be considered to be only a question of time.

THE SOURCE OF THE POWER OF THE TIDE MILL.

From "Engineering."

THE source of the power of the tide mill or the principles concerned in its action would appear to be regarded as a somewhat involved subject—at least it is not generally treated of in the text-books. Mayer's view, as is well known (as given in Professor Tyndall's book "Heat a Mode of Motion," page 433) is that the power of tidal machinery is derived at the expense of the earth's rotation. This is shown by the following passage: "Supposing then that we turn a mill by the action of the tide, and produce heat by the friction of the mill-stones; that heat has an origin totally different from the heat produced by another pair of millstones which are turned by a mountain stream. The former is produced at the expense of the earth's rotation, the latter at the expense of the sun's heat which lifted the mill-stream to its source."*

No explanation of the *mode* of retardation of the earth's rotation by the action of tidal machinery is given in Professor Tyndall's book, or apparently in Mayer's writings—though the mode of retardation of the earth's rotation by the *friction* of the tidal wave is very clearly explained. As therefore we think the subject has some practical interest, and fully admits of elementary exposition, we propose to say a few words on the subject in this article, having previously considered the matter carefully.

The phenomenon of the tide consists, as is well known, in an elevated mass or protuberance of water, situated (approximately) in the line joining the earth and moon, this protuberance of water not being carried round by the rotation of the earth; so that by this rotation, objects are carried through this elevated mass of water or tidal wave, dipping gradually below the wave at one boundary and emerging gradually from the wave at the other boundary, giving rise to what is called the rise and fall of the tide. It is well to keep distinctly in view that (neglecting the relatively slow motion of the moon and only taking into account the phenomenon of the ordinary diurnal tides) the tidal phenomena are not due to the rotation of the tidal wave about the earth, but to the rotation of the earth beneath the tidal wave, which itself is stationary. Thus the rise of the tide on a beach is not due to the movement of the tidal water up the beach, but to the movement of the beach underneath the tidal water. It is important to keep this in view for the proper realisation of the facts.

Let us suppose a reservoir carried round by the earth's rotation beneath the tidal wave, and let the reservoir freely fill and empty itself as it is carried along; then evidently there will be no work done in retarding the earth's rotation, and no work can be got by means of the reservoir. If, however, during the passage of the reservoir through the tidal wave, we impound a portion of the water and let the reservoir (by its rotation with the earth) carry away that portion of water in a direction from the tidal wave (*i.e.*, from the line joining the earth and moon), then this portion of water must be carried away in opposition to the pull of the moon which tends to retain it in the tidal wave; and since this carrying away of the water is done by the earth's rotation (carrying the reservoir with it) it follows that the earth's rotation is thereby retarded; just in the same way as it would be if we imagine that the entire tidal wave had been impounded and carried round with the earth, away from the line joining the earth and moon, which it tended to approach under the influence of the moon's attraction. What applies to the entire wave or mass of water applies to any portion of it, only the above extreme case may serve to put the fact of the retardation of the earth's rotation in a striking light.

If the water thus impounded be allowed to flow out of the reservoir and descend through a mill, then the power thus derived from the water is the exact equivalent of that abstracted from the earth's rotation. If, on the other hand, the water be retained in the reservoir so

* Mr. Robert Mallet questions this view (*Phil. Mag.*, July, 1874).

as to come round (by the earth's rotation) to the tide wave on the opposite side of the earth, then (conversely) in the approach of the water in the reservoir to the line joining the earth and moon towards which it tends, work will be done by the moon in pulling this portion of water round, so that the earth's rotation will be accelerated by an amount precisely equal to the retardation in the previous case, and, therefore, on the whole, no retardation of the earth's rotation will ensue. At the same time no work is got out of the impounded water; so that, therefore, it becomes clear that in order to derive work from the water, the earth's rotation must be retarded, by an amount equivalent to the work derived.

To look at the same fact from a somewhat different point of view, we may suppose that instead of impounding water, work is derived by raising a floating body by the tide. Then if the rise and fall of the floating body be not artificially resisted, it behaves exactly as the equal weight of water displaced by it would do, and the earth's rotation is not affected in any way, and at the same time no work is derived. But if the rise of the buoyant body be artificially prevented, then the earth in the act of its rotation has to plunge this body below the stationary crest of water raised by the moon's attraction, and in performing this act, the earth has its rotation checked by an amount equal to the work done in plunging the floating body below the surface of the water. The *mode* of retardation of the earth's rotation will, we think, be very clear and obvious in this example.

If after the buoyant body has reached (by the earth's rotation) the highest crest of the tidal wave, and is there totally immersed, the body be then allowed to rise, then in this act the exact energy which was abstracted from the earth's rotation can be utilized for performing work. If then the floating body be allowed to sink freely again with the tide, no further retardation of the earth's rotation will ensue. But if the floating body be artificially confined at its highest level, or prevented from falling with the tide, then the rotating earth will do further work, for in the act of rotating it has to lift this body out of the water; or in other words, the rotating earth in transferring the body (which is not allowed to fall) from a portion of the tide wave where the water is high, to a portion where the water is low, has in that act to lift the body out of the water, for the body is no longer immersed at the part of the wave where the water is low. In this act of lifting the body, the rotation of the earth is retarded by an amount equivalent to the work done in lifting.

If then, after the suspended body has been carried round by the earth's rotation to the boundary of the tidal wave (when the water is at its lowest), the body be allowed to fall, then work may again be derived from the body which exactly represents that abstracted from the earth's rotation in lifting the body out of the water.

The level of the water, it may be observed, is abnormally altered by the action of the moon. If it were not for this action of the moon, the surface of the water would always remain at the same level about objects on the rotating earth, and no immersion and emersion of these objects would take place by the earth's rotation: but on account of the abnormal elevation of the water opposite the moon, alternate immersion and emersion of objects necessarily takes place by the rotation of the earth beneath this elevation of water, and thus this rotation may be readily made to perform work.

Thus we observe that every operation of the tide mill takes energy from the rotating earth, and the inevitable conclusion follows that such operations, if continued long enough, would bring the earth to rest.* The store of energy in the rotating earth is, however, so vast as to be practically inexhaustible by such operations in any time that we take account of. The rotative velocity of the earth's surface is such as to carry objects fixed on its surface, from the

* In regard to the retardation of the earth's rotation through *friction* of the tidal wave (a cause quite different from that we have been considering), it may perhaps be worth noticing that the continual scour produced by the rotation of the earth beneath the tidal wave, may possibly be abrading the equatorial regions of the earth and transferring the abraded material towards the still water at the poles. The tidal wave has been aptly compared to a brake, and is in fact a brake formed of two halves of water inside which the earth rotates (the grip being applied by gravity). Few brakes can operate without a sensible abrading action, and the abrading power of water is well known.

lowest to the highest part of the tidal wave (a distance of one-fourth the earth's circumference at the equator) in six hours, so that a lift can be obtained from the rotating earth (through the mechanism of tidal machinery) at every such interval. It would appear that this vast source of power were deserving of more attention than appears to be accorded to it, or that the problem of using the rotating globe as a driving power through the mechanism of the tide might be worthy of the attention of practical engineers.

PRELIMINARY NOTE ON THE USE OF THE PIEZOMETER IN DEEP-SEA SOUNDING.

By J. Y. BUCHANAN, Chemist to the Challenger Expedition.

Proceedings of the Royal Society.

In order to determine the depth of the sea independently of the length of sounding-line used, piezometers filled with distilled water were frequently attached to the line along with the deep-sea thermometers. The combined effects of change of temperature and change of pressure were registered by a steel index of ordinary form. The temperature of the bottom-water being given by the deep-sea thermometer, the effect of temperature on the apparent volume of water in the piezometer could easily be calculated; and from the residual effect, the pressure, and therefore the height, of the column of water to which the instrument had been subjected could be deduced.

The piezometer did not differ materially from the ordinary ones used for the determination of the compressibility of liquids. A minute description of the fittings necessary for their safe use on the sounding-line cannot be given without reference to a drawing or model, and must therefore be postponed.

It is manifest that if the apparent compressibility of water is accurately known, we shall be in a position to determine, by means of our instrument and a deep-sea thermometer, the depth to which it has been sunk, independently of the lengths of sounding-line used; for the indications of the instrument depend solely on the temperature of the water at the depth in question, and on its vertical distance from the surface.

The determination of the effect of change of temperature on such an instrument does not demand explanation. It is, however, otherwise with the effects of pressure. In submitting an instrument of the kind to high pressures in an hydraulic machine, we encounter difficulty in accurately determining the pressure to which it is exposed, and also, although in a minor degree, in making our observations at the low temperature usually obtaining in deep ocean waters. I have therefore taken, as basis for the determination of the apparent compressibility of water, the results obtained when the instrument has been sent down on ths sounding-line, either to the bottom or to intermediate depths, in positions where there has been no apparent disturbance from currents, and where the amounts of compression produced have been proportional to the depths recorded by the sounding-lines. Where currents are absent, and their presence is at once detected by the behaviour of the sounding-line, the depth, as determined according to the method of sounding adopted on board the "Challenger," gives an excellent measure of the pressure exercised on the instruments. As the variations in the temperature, the salinity, and the compressibility of sea-water with the depth have been thoroughly investigated for the soundings in question, the weight of a column of sea-water in any of these localities can be calculated with great accuracy.

The observations which have been taken as a basis for determinations of depth were made in the latter part of the year 1875 in the South-Pacific Ocean. They were twenty in number, and were made at depths varying from 500 to

2,300 fathoms, and at temperatures varying from 1°.4 to 4°.03 C. The mean compressibility of water determined from these observations was 0.0008986 per 100 fathoms of sea-water, the extreme values being 0.000915 and 0.000882. Observations made at greater depths in the North Pacific gave as a mean of six observations at depths varying from 2,740 to 3,125 fathoms the value 0.000878, indicating a slight diminution in the coefficient of compression at very high pressures.

The effect of pressure being thus known, we are in a position, by comparing the indications of the instrument with those of a trustworthy deep-sea thermometer, to determine the absolute depth to which it has been sunk beneath the surface; and assuming the depth as indicated by the sounding-line to be correct, we should be able to determine the temperature at the depth in question from the indications of our instrument, and without the use of a thermometer. For the latter purpose, however, the instrument, as above described, is useless, because the dilatability of water at the low temperatures, obtaining in deep water, is so small as to be negligible compared with its elasticity.

The application, however, of the principle above indicated would manifestly present some very great advantages in the determination of deep-sea temperatures.

In the open ocean, where, as a rule, the temperature diminishes constantly as the depth increases, the Millar-Casella thermometer gives sufficiently accurate results. In the case of enclosed seas, or in the neighborhood of ice, however, this is not always the case. In the Mediterranean, the Red Sea, and many of the seas of the Eastern archipelago, besides, possibly, large tracts both of the Atlantic and Pacific Oceans, the temperature decreases regularly down to a certain depth, which is different for different seas; and at all greater depths the Millar-Casella thermometer gives identical readings, indicating that the water is either at the same temperature or some higher one. In the neighborhood of ice, layers of water are frequently met with at various depths whose temperature, being higher than that of the surface, is indicated by the maximum index of the Millar-Casella thermometer. Besides these layers there may be, and there probably are, others whose temperature is higher than that of the water immediately above them without reaching that of the surface, and their temperature would remain unrecorded. It would therefore be of great advantage if the piezometer could be adapted for the determination of temperatures at known depths. An efficient instrument for this purpose has been obtained by filling the bulb of the piezometer with mercury instead of water. The portion of the stem in which the index moves is filled with water, and, as in the other, the open end dips into a cup of mercury. We have thus an instrument filled with a very large quantity of mercury and a very small quantity of water; and after immersion the position of the index shows the apparent volume assumed by this mixture under the combined influence of temperature and pressure. As far as the effects of temperature are concerned, the amount of water in the instrument is almost wholly negligible; but when the effect of pressure is considered, the apparent compressibility of mercury is so small, being little more than one-fiftieth of that of water, that the pressure of even so small a quantity of water as can be contained in the graduated tube increases very materially the amount of contraction produced by pressure. The instrument, which has been in use since the beginning of November last year, contains 256.61 grammes of mercury in the bulb and stem immediately above it; the volume of the part of the stem filled with water is 0.1935 c. c. The apparent contraction of this mass of mercury and water is 0.000581 cubic centimeters per 100 fathoms, and 0.0025 c. c. per degree respectively. A fall therefore of one degree in temperature produces the same effect as an increase of pressure equal to 430 fathoms of sea-water. Hence (and this forms the important peculiarity of the instrument) as long as the temperature of the sea does not increase with the depth at a greater rate than 1° C. per 430 fathoms, the instrument will record the temperature correctly. The ratio subsisting between the amount of temperature and the column of water, which produce the same effect on the apparent volume, is a

constant for every instrument; in our one it is $\frac{1}{430}$. By altering only very slightly the amount of water, the sensibility to pressure is greatly increased or diminished, while that to temperature remains practically unchanged. As the instrument was intended principally for bottom-waters, the above ratio ($\frac{1}{430}$) was considered sufficient, and it has proved practically useful. It must be remembered that the greater the value of this ratio is made, the greater is the error introduced into the determination of the temperature by any inaccuracy in the measurement of the depth.

By attaching a combination of one, or better of two, of each of these instruments close to the weight at the end of the sounding-line, the depth of the sea and the temperature of the water at the bottom at any locality can be accurately determined, provided that sufficient evidence is afforded, either by the presence of a sample of bottom in the sounding-tube, or by the rate at which the line runs out, that the instruments have been at the bottom; otherwise the depth which they have attained and the temperature at that depth will be correctly given. For this purpose it is necessary first to let the line run out until, from observations on its velocity, it is evident that the weight has reached the bottom; the length of line which has so run out will give the depth approximately, but more or less in excess of the truth according to circumstances. Allowing for the contraction which would be produced by this depth in the case of the mercury piezometer, a first approximation to the temperature of the bottom-water is at once obtained; and it is sufficiently accurate for the purpose of correctly determining the contraction produced on the water piezometer by the change of temperature, and consequently for deducing the depth to which the instrument has been sunk. By now applying the more correct depth to the reading of the mercury instrument we obtain the correct temperature, and if necessary the approximation might be carried still closer.

As an example of the use of the combined instruments, the observations made on the 29th February, 1876, may be taken. The position of the sounding was lat. 36° 9′ S., long. 48° 22′ W., and the depth by line was 2,800 fathoms. The sea was quite calm, but there was a strong current setting to the south-east, rendering it probable that the depth, as determined by line, was considerably in excess of the true depth. The mercury instrument registered 166.2 millims. In order to clear this reading for a depth of 2,800 fathoms, we have to subtract 16 millims., and we obtain 150.2 millims. as the corrected reading, from which we determine the temperature to be +0°.2 C. The reading of the water instrument was 283.8 millims. Assuming the temperature to have been 0°.2 C., this would indicate that the water had suffered an apparent contraction, owing to pressure alone, of 0.1923 c. c., which would be produced by a column of 2,480 fathoms of sea-water. Assuming now 2,480 fathoms to be the true depth, we find the corrected reading of the mercury instrument to be 152.1 millims., which indicates a temperature of −0°.5 C. The Millar-Casella thermometers gave the temperature as −0°.4. Assuming this as the correct bottom temperature, and reducing the reading of the water instrument accordingly, we find the contraction produced by pressure to be 0.1924 c. c., which agrees sensibly with that found on the assumption of the higher bottom-temperature of +0°.2 C.

It will thus be seen that the two instruments fulfil the conditions required of them; namely, that the one which is to indicate the temperature of the water shall be independent of great accuracy in the determination of the depth, and the one which is to indicate the depth shall be equally independent of accurate determination of the temperature; whilst by combining the results obtained by the two, an accurate determination is obtained both of the depth and of the temperature of the water at that depth.

It may not be generally known that Prof. J. C. Poggendorff died in Berlin on the 24th of January last, at 81 years of age. In 1821 he wrote his treatise "On the Magnetism of the Voltaic Pile." In 1824 he was appointed editor of the *Annals*, a position which he retained until his death. He filled a chair in the University of Berlin since 1834, and was a member of the Berlin Academy since 1838.

PHOSPHORUS AND IRON.

From "The Engineer."

THE Bessemer process has gone far to ruin the iron trade of England. While a good steel rail can be had for about £6 10s. per ton no one will buy iron rails, costing £6 and lasting one-third of the time. It is not impossible that steel boiler plates, and even ship plates, may yet be produced at a cost little exceeding that of iron, while they will be comparatively free from the uncertainty of quality which now militates against the extended substitution of steel for iron. We could, indeed, hardly be charged with rashness if we said that in a few years steel might so far have taken the place of iron, as a constructive material, that the latter would be made only on a small scale, and of the best quality for a few special purposes. It is well, however, to bear in mind that the position which the steel manufacture has assumed is due as much to the low price at which the metal can be produced as to the qualities of the finished article. It is not to be denied that there are very bad steel rails made and sold, and it is well known that much so-called steel is really iron produced by the Bessemer or Siemens-Martin process. That is to say, it will not harden, in the proper sense of the word, when heated and dipped in cold water, and its tensile strength and elongation under strain are more nearly those proper to good wrought iron than comports well with the popular idea of the characteristics of steel. The process, in a word, gives the product a name. Be this as it may, the growing popularity of steel, or so-called steel, is so rapidly destroying the iron trade, especially in rail-making districts, that it is beginning to be accepted as proved that a shire or a county which does not possess ores suitable for the production of Bessemer pig cannot hope to carry on a successful trade in rails, at all events; while it can hardly be found worth while to manufacture iron plates or bars alone in their absence, save in isolated works. Unfortunately for the Cleveland district, this is precisely the plight in which it stands at present. Possessed of enormous quantities of ore, while coal can be had in abundance at low prices, Middlesbrough and the surrounding country are reduced to sore straits because they cannot make steel rails. The defect in Cleveland pig is very easily stated. It contains, besides other impurities, far too much phosphorus. Rails or bars made from it are consequently cold short to a dangerous degree; and it has become with the Cleveland ironmasters almost a question of life and death to clear their iron of the element which poisons it. It is no wonder, therefore, that energetic efforts are being made to discover some method by which the phosphorus may be eliminated. A great deal of money has been spent in this direction with little success. The last laborer in the field is Mr. I. Lowthian Bell, and he availed himself of the recent meeting of the Iron and Steel Institution at Newcastle to place his theories and proposals before the world. We have already published the discussion which followed on the reading of his paper, and that of one by Mr. Siemens. It is matter for regret that two gentlemen, so able, should have confined their attention during the discussion to matters which did not throw much more light on the subject they had in hand. We have already given an abstract of Mr. Siemens' paper, and we propose here to consider Mr. Lowthian Bell's communication. This last we have not published, because, in the first place, it is very long; and, in the second place, it is somewhat difficult to follow its author's line of reasoning. To abstract it would be impossible; but we can explain here very briefly how Mr. I. L. Bell proposes to free Cleveland iron of its phosphorus; and that, after all, is the most important question.

Last March, Mr. Lowthian Bell read a paper in London before the Iron and Steel Institute, in which he showed that carbon and silicon were expelled from iron at moderately high as well as at excessive temperatures, oxygen being the eliminating agent. The paper with which we are now dealing is a sequel to that read in London, and in it Mr. Lowthian Bell states that phosphorus does

not behave in quite the same way as carbon and silicon, experience acquired with the Bessemer converter proving that oxygen in its free state, or in oxide of iron, was entirely unable to remove phosphorus at exalted temperatures; that is to say, at these heats phosphorus and oxygen cannot unite, and the former element remains combined with the iron. To put this proposition into somewhat more popular language, although carbon and silicon can be burned out of iron at almost any heat within a wide range of temperature, phosphorus cannot. It remains to be seen if it can be burned out at any temperature. Mr Lowthian Bell states that it can. "When," he says, "melted pig iron *was exposed to the action of fluid oxide of iron at lower temperatures phosphorus was rapidly removed.*" We have put this passage in italics because it contains the essence of Mr. Lowthian Bell's discovery. The lower temperature is one somewhat less than that at which iron is puddled, very much less than that in a Bessemer converter while a charge is being blown. If Mr. Lowthian Bell is right, then his discovery ought, if it can be applied in practice, to revolutionise the trade of Cleveland. For that district he will have done more than Mr. Bessemer did for the world. It remains then to be seen whether his deductions are sound, and whether the process can readily be applied under ordinary manufacturing conditions.

As regards the first point, we have no reason to question the accuracy of Mr. Lowthian Bell's views. He seems to have carried out his experiments very carefully. He first ascertained, for example, that at moderate heats he could get rid of phosphorus, and then he found out that at high temperatures the metal would take up the phosphorus again. In one instance a specimen of Cleveland iron had been so well freed from its phosphorus that only .055 per cent. of this substance remained. A portion of this purified metal was subsequently exposed to a strong heat, about equal to that of a puddling furnace, in contact with the cinder used in its purification. At the end of sixty-five minutes the phosphorus had risen to .153 per cent., and in three hours the iron contained .365 per cent. of this element. Results like these are conclusive. The moment Mr. Bell had thus clearly opened his subject and laid down his propositions, he went out of his way to describe the results obtained with iron and steel rails made by different processes, and we have, in consequence, a great mass of figures and facts, which, however interesting, are really in a great measure beside the question, and break the thread of his discourse in a very disastrous fashion. It was hardly necessary, we think, to explain and prove at great length to the members of the Iron and Steel Institute that phosphorus was an extremely objectionable ingredient in a rail or a bar; Cleveland requires no evidence on this subject. Thanks to this digression, it is not until nearly at the end of the paper that we have any explanation of the way in which it is proposed to apply Mr. Lowthian Bell's discovery in practice. There we learn that he is engaged at present, at the request of the North Eastern Railway directors, in erecting proper apparatus for carrying on the necessary experiments on a practical scale. Upon an elevated platform a small cupola will be placed, in which the cinder or other form of oxide will be melted. From this the fused material will be run into a vessel revolving on its centre, into which the liquid iron direct from the blast furnace will also be introduced. It is expected that in this way a sufficient blending of the two fluids will take place to effect the object had in view. One of Messrs. Godfrey and Howson's puddling furnaces will be used for the revolving vessel. Its construction will enable the interior to be heated expeditiously when required, and thus avoid a cold surface cooling the materials before the necessary change is effected, while the power of inclining the vessel at an angle will permit the process to be continued to a point which must be determined by the extent to which it is wished to remove the carbon.

The information contained in the foregoing passages is meagre enough. It is clear, however, that the process consists in washing the melted iron in a cinder bath, which will extract the phosphorus. The cinder will, we presume, be subsequently tapped off, and the iron puddled in the revolving furnace. On this point we are not quite clear, however; that is

to say, we are not certain whether the iron, when washed, will be withdrawn from the rotator and puddled in the ordinary way, or whether the process of washing and puddling will both be conducted in a single vessel. Possibly this is a detail to be settled subsequently. The essence of the process consists, as we have said, in the use of melted oxide of iron as a detergent fluid. Mr. Lowthian Bell, very properly, was careful to point out that it had long been well known that a cinder bath will remove phosphorus. He is, however, apparently among the first, if not the first, to prove that the washing process can only be carried on at a moderate temperature; that is to say, just at or about the point of fusion of pig iron. Now, it is difficult to see how the carbon in the pig can be brought into contact with the oxygen in the bath without being burned out, but as it goes away the fusing temperature of the iron will rise, until a point is reached at which the phosphorus could be taken up again, while lacking this heat the iron would solidify in the hearth. This may prove a source of trouble, unless all the phosphorus can be expelled before the carbon is seriously attacked by the oxygen. It is fair to add that Mr. Lowthian Bell speaks very modestly indeed concerning the whole matter. "My plea," he said, "for occupying the attention of the members of the institute is the pains I have taken to discover the natural laws which govern the affinity between the metalloids, referred to in the title of this paper, and iron. The effect of these inquiries has been to prove that by substituting moderate for the more elevated temperatures in use in our forges, phosphorus has been made to change places with the carbon in the order in which these two bodies leave the iron. Whether this fact will have any value beyond that which accompanies any contribution, however slight, to our knowledge on these subjects, remains to be proved. Its practical, or, in other words, its commercial importance, must depend upon the expense of removing the impurities from those cheap varieties of pig iron in which they occur." As bearing on the last few lines in the foregoing passage, we may extend the hypothesis of Mr. Stead, who took part in the discussion, and suggest that perhaps the cheapest way of applying the principle would consist in working a double-puddling furnace, very heavily fettled, at a low temperature for seven or eight minutes after the iron was all melted, and then running off nearly the whole of the cinder, and completing the process of puddling almost on a dry bottom. Much the same thing is done now, but the cinder is not present in sufficient quantity, the temperature is allowed to get too high in the earlier stages of the process, and the cinder is run off too late. The result is that much of the phosphorus taken out of the iron at first is restored to it subsequently.

OTTO'S NEW GAS ENGINE.

Translated from "Le Gaz."

THIS new gas motor bears no resemblance to the other and older engine which bears the name of this inventor.

It consists simply of a piston raised by the explosion of a mixture of gases below it, and which is aided in its descent by atmospheric pressure, there being a vacuum below.

It is silent in its action, working horizontally much after the manner of the Lenoir engine, but upon another plan of action which we will proceed to describe. Like the vertical engine its economy of working is well established, a cubic meter of gas, it is said, supplying the work of a horse-power per hour. It seems certain from observation that in the matter of regularity of action it leaves nothing to be desired.

The construction is simple. It consists of a working cylinder closed at one end, and within which moves a piston which is attached by a connecting rod to the crank shaft. Upon the shaft are the fly-wheel and working pulley.

The piston does not descend to the

bottom of the cylinder; the extra length of cylinder being equal to about half the stroke.

Suppose the engine working and the piston at the bottom of the stroke; the lower space is then filled with the gaseous products of the combustion as will be presently explained; the movement of the piston continues by reason of the momentum of the fly-wheel tending to suck in air until it has made half of a stroke. At this time the explosive mixture is introduced.

We have then in the cylinder three distinct sections sensibly equal, composed as follows: the first, next to the piston is a mixture of air and products of combustion; the second is pure air; and the third, at the bottom of the cylinder, is an explosive mixture of air and gas.

The movement of the engine continuing, the piston returns, compressing these three gaseous layers in such manner that when it has completed the stroke, the total volume has been reduced to one-third its original bulk, and consequently the gases are under a pressure of three atmospheres. At this moment the bottom layer, the explosive mixture, is exposed to a gas flame. The explosion is more or less rapid according to the mixture. The heat of the explosion causes great tension of the gases acting upon the piston during its third stroke, and during this stroke only is there any work imparted to the moving parts.

The piston having completed its upward stroke returns, driving before it the products of combustion, which escape by a valve till the piston has completed its fourth stroke and is in the position in which we began the analysis of its action.

Thus, after the work afforded by the combustion of the gases has been expended on the piston during one of the strokes, the piston makes three consecutive strokes without further impulse, and has besides to condense the gaseous mixtures to a tension of three atmospheres.

This action requires the function of a heavy fly-wheel, which further augments the importance of the passive resistances which are already very great, inasmuch as the piston is urged during a fourth only of its course by the moving agency of the machine.

In order to explain the better efficiency of this engine compared with the Lenoir engine, it is necessary to analyze the action of the latter.

The Lenoir engine receives the explosive mixture in the body of the pump. The explosion produces a very high temperature, and the escape of heat through the enveloping surfaces is very rapid. The piston moves but a short distance before a sensible loss of heat has occurred, and this of course implies a corresponding loss of tension of the gas. Furthermore, the heating of the pumps requires a cooling process by aid of a current of water. So that a large percentage of the energy of the combination is forcibly abstracted.

In the new Otto engine the heat of combustion is divided between three volumes; one of these, it is true, is at a very high temperature, but the other two are at the temperature of the surrounding objects, and after the explosion they attain a degree of heat only about half that of the exploded mixture of the Lenoir engine. This reduction of the temperature leads to another loss in dynamic effect, for if the tension is reduced to one-half, the volume of the compressed gas is doubled. At the temperature thus reduced, the loss by radiation and the contact with the sides of the cylinder, diminishes in a proportion greatly to the advantage of this motor.

It would be desirable in Otto's engine to relieve the cylinder entirely of the combustion products after they have acted on the piston, if they could be replaced by the same volume of air. The same theoretic effect would be obtained at a less temperature, and as the loss by radiation and by contact would be less the useful effect would be augmented.

Some details of the engine require description. M. Otto provides at the bottom of his cylinder a little cylindrical chamber of fifteen or twenty centimeters in length. At the bottom of this cylinder is the opening by which enter in succession the air and the explosive mixture of gases. The advantage of such an arrangement is evident; the mixture is preserved more intact than it would be in the larger cylinder and the ignition is managed with more certainty.

We have shown how that in the working of this engine, its action varies for

four successive strokes. Thus it is necessary—

1st. That during the first stroke, the cylinder should admit during the first half, air and during the remainder the explosive mixture.

2d. That at the end of the second stroke the explosive mixture should be ignited.

3d. That at the end of the third stroke, and during all of the fourth, a valve should open to permit the escape of the products of the combustion and the residual gases.

To accomplish these results M. Otto has furnished his engine with a counter shaft moving with half the angular velocity as the main shaft, thus making a single revolution for the four strokes of the piston.

This counter shaft carries a rod communicating with a slide at the bottom of the cylinder which is held open during a fourth of a revolution or during one stroke, and so exposes the gas flame to the atmosphere.

A coupling upon this shaft also carries a cam which regulates the flow of gas from its reservoir.

By the same connection also is opened the valve for the admission of the explosive mixture. The coupling slides along the shaft and is guided by a regulator. When the motion is accelerated beyond certain limits the cam is so displaced that the inlet gas-valve does not open, and the explosion of the gases is omitted till after the next four strokes.

The little gas burner, which ignites the mixture at the proper moment, is so placed as to be subjected to the pressure of three atmospheres, and therefore requires a special adjustment.

Another coupling and cam opens the valve for the escape of the residual gases.

FOUNDATIONS.*

By JULES GAUDARD, Civil Engineer.

From Proceedings of the Institution of Civil Engineers.

I.

The Author proposes to give a description of the principal methods resorted to in making foundations. Although these methods are applicable, in general, to every sort of construction, they possess a special importance in the case of large bridges, on account of the greatness of the load, the instability of the soil, and the amount and flow of water to be contended with. It is not sufficient, moreover, that the bed of the river and the ground upon which the foundations of a pier rest are firm, they must also be secured against scour, as only hard rocks are unaffected by a rapid stream. To ascertain the nature of the soil on which foundations are to be laid borings are generally taken, but they sometimes prove deceptive, owing to their coming on some chance boulders, or upon some adhesive clays which, without being firm, stick to the auger, and twist it, or arrest its progress, and the specimens brought up, being crushed and pressed together, look firmer than they really are. To remedy these defects some engineers have adopted a hollow boring tool, down which water is pumped and reascends, by an annular cavity between the exterior surface of the tool and the soil, with such velocity that not only the detritus scraped off by the auger, but pebbles also, are lifted by it to the surface. This process is rapid, and the specimens, which are obtained without torsion, preserve their natural consistency.

On stiff clay, marl, sand, or gravel, the safe load is generally from 55 to 110 cwt. on the square foot, but a load of 165 to 183 cwt. has been put upon close sand in the foundations of the Gorai bridge, and on gravel in the Loch Ken viaduct and at Bordeaux. In the bridge at Nantes there is a load of 152 cwt. to the square foot on sand, but some settle-

* Translated from the French by L. F. Vernon-Harcourt, M.A., M. Inst. C.E.

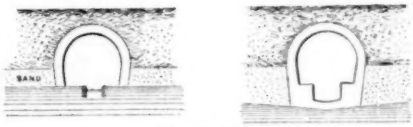
ment has taken place. Under the cylindrical piers of the Szegedin bridge in Hungary, the soil, consisting of clay intermixed with fine sand, bears a load of 133 cwt. to the square foot; but it was deemed expedient to increase its supporting power by driving some piles in the interior of the cylinders, and also to protect the cylinders by sheeting outside. Cylinders, moreover, sunk to a considerable depth in the ground, possess a lateral adherence, as is evident from the weights required for sinking them, which adds greatly to the stability of the foundations. Taking into account this auxiliary support, the loads of 159 and 117 cwt. per square foot, at the bottom of the cylinders of the Charing Cross and Cannon Street bridges respectively, are not excessive. On a rocky ground the Roquefavour aqueduct exerts a pressure of 268 cwt. to the square foot.

Foundations may be classed under two heads:—(1) Ordinary foundations, on land, or protected from any considerable rush of water; (2) Hydraulic foundations, in rivers, or in the sea.

ORDINARY FOUNDATIONS.

When the ground consists of rock, hard marl, stiff clay, or fine sand, the foundations can be laid at once on the natural surface, or with slight excavation, and with horizontal steps where the ground slopes. At the edge of steep descents, with dipping strata, it is necessary to find layers which will not slip, or, if there is such a tendency, to strengthen the layers of rock by a wall, especially when it is liable to undergo decomposition by exposure to the air, or to use iron bolts uniting the layers of rock. On ground having only a superficial hard stratum resting upon a soft subsoil, buildings have sometimes been erected by merely increasing the bearing surface, and lightening the superstructure as much as possible; but generally it is advisable to place the foundations below all the soft soil. On an uneven surface of rock a layer of concrete spread all over affords a level foundation. Sometimes large buildings have been securely built on quicksands, of two great thickness to be excavated, by the aid of excellent hydraulic mortar, and by excavating separately the bed of each bottom stone. Such a building will be stable if its pressure on the foundation is uniform throughout, and if it is placed sufficiently deep to counterbalance the tendency of the sand to flow back into the foundations. Instances of this class of foundations are to be found in sewers built on water-bearing sands, which sometimes give rise to as much difficulty as foundations built in rivers; as for example in the net-work of London sewers, and in the Metropolitan railway. The flowing in of sand with the water in pumping, and consequent undermining of the houses above, was prevented in these cases by constructing brick or iron sumps for the pumps in suitable places, surrounding them by a filtering bed of gravel, and using earthenware collecting pipes, thus localising the disturbance. In the construction of the Paris sewers, where the water-bearing strata could not be excavated on account of the running in of the sand, the upper portion only of the culvert was first constructed (Fig. 1). A little trench was then dug

Fig. 1. Fig. 2.



SEWERS IN PARIS

out at the bottom, each side being supported by interlaced boards, and this trench was then pumped dry in lengths of about 110 yards at a time. When one length was dry, a second row of boards was beaten down on the top of the first row, and at last it was possible to excavate the soil in lengths of thirteen feet, carefully shored up, in which the lower portion and the invert could be constructed, completing the section of the culvert (Fig. 2). In this manner a culvert, nine feet ten inches wide, twelve feet six inches high, and 2,950 feet long, was constructed in eighty-five days. The excavations for a sewer at Grenoble were executed from below upwards, in order to insure a continuous flow of the water, and the sides were built as the excavation proceeded, a trench supported by boards conveying the water, and the invert was begun when the piers were finished, commencing at the upper part;

semicircular troughs of cement having been placed at the bottom of the excavation to afford continuous drainage, over which a layer of quick-setting concrete was deposited (Figs. 3 and 4).

Fig. 3.

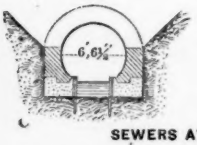
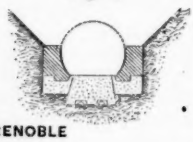


Fig. 4.

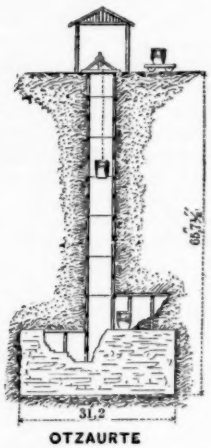


SEWERS AT GRENOBLE

One means of reaching a solid foundation without removing the upper layer of soft soil is by piling, but piles are liable to decay in many soils. In Holland, buildings on piles of larch, alder, and fir have lasted for centuries, whilst in Belgium large buildings have been endangered by the decay of the piles on which they rest. Sometimes columns of masonry support the superstructure, but, being placed farther apart than piles, it is necessary to connect them with arches at the surface for carrying the walls. Piers, however, of viaducts supporting a heavy load must be carried down in one mass to the solid ground, as in the case of the viaduct of Otzaurte, on the Rio Salera in Spain, where it was necessary to get through sixty-five feet of silty clay to lay the foundations of a pier thirty-one feet long by thirteen feet wide. In order to avoid getting out so large an excavation in one piece, a well was dug, four feet wide, and extending across the whole width, thirteen feet, of the pier, so as to divide it into two equal portions (Fig. 5). A chamber, nine feet ten inches high, was then driven at the bottom, like a heading, as far as the limits of one-half of the foundation of the pier, and built up with masonry. The other half was similarly dealt with, and the excavation and masonry were carried up in successive lifts of nine feet ten inches. The central well served as a means of access for pumping out the water, for the removal of earthwork, and for the supply of materials.

To avoid the difficulty and expense of timbering deep foundations, a lining of masonry is sometimes sunk, by gradually excavating the ground underneath, and weighting the masonry cylinder, which is eventually filled in with rubble stone, concrete, or masonry, and serves as a pier.

Fig. 5.



OTZAURTE

In India a similar system has been followed for centuries for sinking wells. The linings are made in radiating courses of bricks or stones; the first length, from five to ten feet high, being put on a circular wooden framework placed on the surface of the ground. Very fine sand is used for filling the joints, except for the two or three top courses, which are laid in mortar, and the whole construction is tightly bound together. It is then gradually sunk by a man inside undermining it, and another length is placed on the top. As these operations are generally conducted in the silty or sandy bed of rivers which become dry in summer, there is no running water to contend with, but water percolates into the excavation, and then the natives use a "jham," by which they remove the earth from under water. Although the external diameter of the wells has been sometimes limited to five feet, the advantage of larger dimensions in securing a vertical descent has been always recognized. At the Western Jumna canal rectangular linings were adopted with advantage. At the Solani aqueduct hollow cubes with sides twenty feet long, and at Dunowri oblong or square linings, thirty feet long and twenty feet deep, and subdivided into three or four compartments, were used.

When the stratum of soft soil is too

thick for the foundations to be placed below it, the soil must be consolidated; or the area of the foundation must be sufficiently extended to enable the ground to support the load. The ground may be consolidated by wooden piles; but in soils where they are liable to decay, pillars of sand, or mortar, or concrete, rammed into holes previously bored, may be used. Artificial foundations are also formed by placing on the soft ground either a timber framework, surrounded occasionally by sheeting, or a mass of rubble stone, or a layer of concrete, or a thick layer of fine sand spread in layers eight to ten inches thick, which, owing to its semifluidity, equalizes the pressure. A remarkable example of this method was afforded in the restoration, in 1844, of the arched way at the Phillippeville gate, at Charleroi, where the old pile-work foundations had twice given way. A trench was dug 3¼ feet deep and 3¼ feet wider than the construction on each side, and inclosed by little walls. Into this cavity was put very fine sand, moderately wetted, then a layer of concrete, twenty inches thick, and upon this the masonry was built, which has stood perfectly. When the bottom of the excavation is silty, it is advisable to throw a thick layer of sand over it before driving piles, as the sand gives consistency to the silt.

A heavy superstructure is partially supported on a soft foundation by the upward pressure due to the depth below the surface to which it is carried, in the same manner that a solid floats in a liquid when it displaces a volume of water equivalent to its own weight. According to Rankine a building will be supported when the pressure at its base is $wh\left(\frac{1+\sin\phi}{1-\sin\phi}\right)^2$ per unit of area, where h is the depth of the foundation, w the weight of the soft ground per unit of volume, and ϕ the angle of friction.

Mr. McAlpine, M. Inst. C.E., in building a high wall at Albany, U.S.A., succeeded in safely loading a wet clay soil with two tons on the square foot, but with a settlement depending on the depth of the excavation. In order to prevent a great influx of water, and consequent softening of the soil, he surrounded the excavation with a puddle trench, ten feet high and four feet wide, and he also spread a layer of course gravel on the bottom.

When the foundation is not homogeneous it is necessary to provide against unequal settlement, either by increasing the bearing surface where the ground is soft, or by carrying an arch over the worst portions.

HYDRAULIC FOUNDATIONS.

Under this head are comprised all foundations in rivers, and where running water has to be contended with.

Foundations are laid upon the natural surface where it is rocky, also on beds of gravel, sand, or stiff clay secured against scour by aprons, sheeting, rubble stones, or other means of protection. When the foundations are to be pumped dry, dams are resorted to if the depth of water is less than ten feet, and are specially applicable to the abutments of bridges, where the water is less deep and rapid and the bank forms one side of the dam. The dam can be made of clay, or even earth free from stones and roots, with slopes of 1 to 1; the width at the top being about equal to the depth of water when the depth does not exceed three feet in a current, or ten feet in still water. The leakage of a dam and the danger of breaches increase rapidly in proportion to the head of water. At Hollandsch Diep a great dam of sand, protected from the waves by fascines, had to keep out a head of water of twenty-three feet at high tides from the foundations. M. de la Gournerie constructed a temporary dam of silt, 4,265 feet long, at St. Nazaire, in 1849, to protect the shed of the floating dock. The dam was thirteen feet high, four feet wide at the top, with a pitched slope of 1 in 3 towards the sea, and an inner slope of 1 in 5.

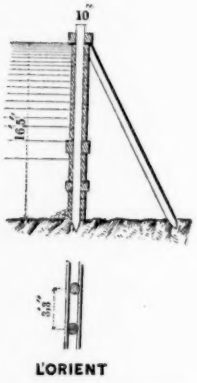
Concrete makes a solid dam, but it is expensive to construct and difficult to remove. A masonry dam 328 feet long was built at Lorient in 1857.

A cofferdam with a double row of piles takes up less space and is less liable to be worn away or breached than an earthwork dam. At the Auray viaduct a dam was made of two rows of piles, with boards filling up the spaces between the piles, the center of the dam being filled with well-punned silt, and protected outside with rubble stones. It supported

the pressure of a head of water of from five to eleven feet; its average cost was £1 0s. 10d. per lineal foot. The width of a cofferdam is often as great as the head of water; but if the cofferdam is strutted inside, so that the clay merely acts as a watertight lining, the width need not exceed from four to six feet. In a cofferdam of concrete at Marseilles constructed for the basin of the graving docks, the widths were calculated at 0.45 of the total height, the maximum width has thus attained twenty feet.

In building the viaduct of Lorient, on a foundation dry at low water, a single row of strutted piles, 3¼ feet apart, planked from top to bottom on both sides, was used (Fig. 6), and the space

Fig. 6.

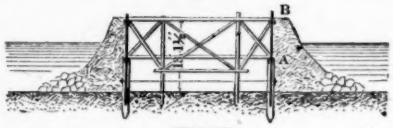


LORIENT

between the planking, ten inches wide, was filled with silt pressed down. When the filling is so much reduced in thickness the planks are carefully joined, and the clay is mixed with moss or tow, or some times with fine gravel or pounded chalk. As water leaks through joints and connections, the ties are placed as high up as possible, and the bottom is scooped out or cleaned before the clay is put in. When the sides of the part to be inclosed are sufficiently close they may be effectually supported by a series of stays, as was done in making the dam for the construction of the apron of the Melun dam (Fig. 7), where struts were put in at intervals of 16½ feet.

The Grimsby Dock works, and the Thames Embankment works, furnished examples of cofferdams constructed to bear the pressure of a great head of water. For constructing the Zuider Zee locks on the Amsterdam Canal a circular dam, 525 feet in diameter, was erected, consisting of a double row of sheet piling, the piles being one foot square and fifty feet long, with walings attached.

Fig. 7.

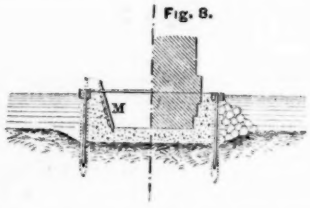


MELUN

Eventually, in consequence of accidents, a third row was added, and the dam further strengthened by sloping banks of sand on both sides, the outer slope being protected by clay and fascine work. The head of water against the dam was occasionally twenty feet.

If large springs burst out in an excavation they must be either stopped up with clay or cement, or be confined within a wooden, brick, or iron pipe in which the water rises till the pressure is equalized, and then it is stopped up as soon as the masonry is sufficiently advanced and thoroughly set. If, however, there is a general leakage over the whole bottom of the excavation it must be stopped by a layer of concrete, incorporated with the foundation courses (Fig. 8).

Fig. 8.



Cofferdams or troughs of concrete had been used on a large scale at Toulon and Algiers for the construction of repairing docks.

Where there is not space for a clay dam, timber sheeting well strutted and caulked is used. For instance at the Custom-house quay of Rio de Janeiro a dam of square sheet piling, with counterforts of cross sheet piling, and made watertight by hoop iron let into grooves in each pile, served to support the press-

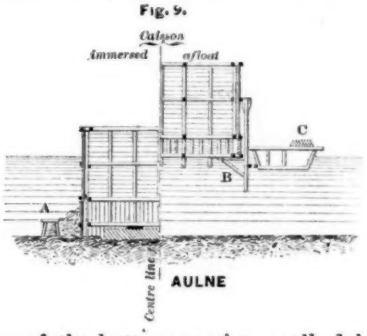
ure of about twenty-three feet of water. A similar structure, however, at the West India Docks was floated away by an equinoctial spring tide, owing to the want of tenacity of the ground. When the head of water is under five feet, tarred canvas is sufficient to keep it out, the canvas being weighted at the bottom, and nailed to a beam at the top. It is in every instance advisable to take out the earthwork for foundations in lengths.

In the construction of the Victoria Docks a metallic cofferdam was used, which was very easily displaced by floating.

Hollow timber frames without a bottom, and made watertight at the bottom after being lowered by concrete or clay, are suitable in water from six to twenty feet deep on rocky beds, or where there is only a slight layer of silt. This method was resorted to by M. Beaudemoulin, between 1857 and 1861, at the St. Michael, Solferino, Change and Louis Philippe bridges at Paris. The timber frame at the St. Michael bridge was fifteen feet nine inches high, 125 feet long, and nineteen feet eight inches wide at the base, with a batter of 1 in 5; the uprights were six inches square, and 6½ feet apart; the framework was made of oak, and the planks of deal (nine inches by three inches), the spaces between them being covered by small laths nailed on to the planks. Fourteen crabs placed on four boats supported the framing, and let it down as it was built up; this was weighted with stones to sink it on the foundations prepared by dredging, and the planks were then slipped between the walings and beaten down lightly. A toe of rubble stone outside supported the pressure of the concrete inside. The whole operation took ten days, and in one month the masonry was finished up to the plinth. The caisson, including erection, cost £560.

The caissons of the bridges at Vienna, sunk twelve feet below water level, cost £2 18s. 6d. per lineal yard of circumference. At the Point-du-Jour viaduct the caissons were 131 feet long, and from twenty-six to thirty-three feet wide, and from twenty-one to twenty-six feet high. The long sides were put together flat on the ground, and were lifted up to allow of the short sides being fixed to them. A few hours sufficed for depositing the caisson in its place. M. Picard in reconstructing the Bezons bridge, after the war of 1870, used caissons in two portions, as the lower portion had to remain, whilst the upper portion was only needed for a time. Some nails and straps fastened the two parts together. A layer of clay was placed under the rubble toe outside, to prevent leakage between the concrete and the planks. This expedient was first adopted by M. Desnoyers, in order to pump dry the foundation which he carried down into the clay, so as to build masonry walls on the bottom without using concrete. At the Aulne viaduct in Brittany, MM. Desnoyers and Arnoux made a caisson seventy-five feet six inches by thirty-four feet nine inches, and nearly twenty-three feet high (Fig. 9), and, with the exception of the bottom portion, caulked beforehand. When it was deposited the bottom planks were slid down between the lower set of walings, and a toe of puddled clay "A," protected from the rush of the current by canvas, was put round the bottom outside. The caisson was so watertight that a Letestu pump working two or three hours each day kept the foundations perfectly dry. When the caisson, put together on a stage supported on eight boats, was ready for depositing, the sluice doors of the Guily-Glas dam were opened, lowering the caisson till the projecting pieces "B" touched the ground, and by cutting the beams fastening these projections to the boats, the boats were set free. As the tide rose the caisson floated, and the boats were attached to its upper part, which, by lightening, lifted it sufficiently for the projecting pieces to be taken off. The depositing was completed by open-

Fig. 9.



ing the sluices of the dam at high water, and as the water fell the caisson, weighted with rails, sank on the dredged bottom. Thus by the help of water alone a mass weighing seventy-four tons was safely and accurately deposited. The cost of one caisson was £740; and the cost of the foundation below low water did not exceed £1 12s. 6d. per cubic yard. At Lorient large caissons, from twenty-three to twenty-four feet high, were employed; but an interior dam of concrete forming a permanent part of the foundation was used instead of an external toe of clay. At Quimperlé M. Dubreil made the caisson watertight by a dam of clay inside, which necessitated a somewhat larger caisson, but admitted of the removal of the timber.

When a limit to the space occupied is immaterial, as on the large American rivers, a sort of double-cased crib-work dam is frequently adopted. M. Malézieux has given various details of this class of work, such as the cofferdam in Lake Michigan to obtain the water supply for Chigaco. A caisson 200 feet long and 98 feet wide, inclosed by double watertight sides from thirteen to nineteen feet high, was used at Montreal on the St. Lawrence. The interval between the two sides was about eleven feet wide, and planked at the bottom so that the caisson could be floated into place. When the caisson was sunk, piles were driven in holes made in the bed of the river to keep it in place, and the bottom was made watertight by a lining at the sides of beams and clay. These kinds of caissons are only suitable where the bottom is carefully levelled. Although iron caissons are generally used for penetrating some distance into the soil, there are instances of iron caissons being merely deposited upon the natural bed. M. Pluyette founded one of the large piers at Nogent sur-Marne in a plate iron caisson, which weighed about seventy tons, and cost £3,600, with a bed of concrete in it ten feet thick and protected by rubble stone. Its dimensions at the bottom were seventy-two feet by 37¾ feet, with rounded corners and a batter of 1 in 15, 29½ feet high, including a length of five feet, which could be removed when the work was finished. The thickness of the plates was from ⅛ inch to ⅛ inch, and it was strutted inside with timber. The same system was adopted at Brême, where caissons sixty-nine feet by 16½ feet were used for the four ordinary piers, and the width increased to 42½ feet for the pier on which the bridge turns; their height was 11½ feet, and the thickness of the plates ⅜ inch. The operation of sinking the caissons from a floating stage occupied about seven hours. A layer of concrete nine feet thick was spread over the bottom and left for twelve weeks to set before the water was pumped out.

The methods employed for laying foundations in the water, either on the natural surface or after a slight amount of dredging, have next to be considered.

A rubble mound foundation is sometimes employed for dams where any settlement can be repaired by adding fresh material on the top; also for landing-piers in lakes by solidifying the upper portion with concrete, and in breakwaters where a masonry superstructure is erected on the top. Such a method, however, is not suitable where a slight settlement would be injurious; and in the sea the base of the mound is generally less exposed to scour than in a river.

Another method consists in sinking a framing, not made watertight, inside which concrete is run, and the framing remains as a protection for the concrete, and is surrounded by a toe of rubble. If the framing is of some depth iron tie-rods are put in by divers after the bottom has been dredged, to enable the framing to support the pressure of the concrete. When piles can be driven the framing is fixed to them. The piles, five to eight feet apart, have a double row of walings fixed to them, between which close planking is driven, from ten to fourteen inches wide, and from three to five inches thick, and sometimes, when the scour of a sandy subsoil has to be prevented, the planks are grooved and tongued, or have covering pieces put on by divers, or are driven in close panels. The insufficiency of a simple framing of planks for foundations on running sand was demonstrated by the destruction of the Arroux bridge at Digoin, and the Gue-Moucault bridge over the Somme by the flood of September 1866, in spite of the fascines and

rubble stone protecting their piers, owing to the washing out of the underlying sand through small interstices by the rapid whirling current. The cost per superficial yard of a casing formed with piles and planks is about £ 1, including the cost of driving 6½ feet.

Open framing is sometimes used for inclosing a mound of rubble stone. These mounds require examination after floods, and renewing till the mound has become perfectly stable.

In permeable soils foundations of concrete inclosed in frames are frequently employed, as, for instance, for the foundations of the Saints Pères, Jena, Austerlitz, and Alma bridges at Paris; but in silty and watertight soils foundations in excavations pumped dry are preferable.

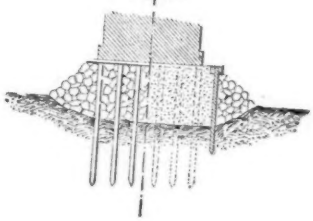
The bed of the Rhone at Tarascon, consisting of sand and gravel in which piles are difficult to drive, is subject to scour in floods to a depth of 46 feet. Foundations, however, were laid there, at considerable expense, by frames with double linings, ten feet apart, in which large blocks were placed with unhewn stones on them; the ground was then dredged inside the frames to twenty-eight feet below low-water level, and 260 cubic yards of concrete were deposited in twenty-four hours.

Lastly, concrete can be deposited *in situ* for bridge foundations; and though concrete blocks are only used in sea works, bags of concrete, like those at Aberdeen, by Mr. Dyce Cay, M. Inst. C.E., might be sometimes employed, instead of rubble stones, for forming the base of piers or for preventing scour.

Piles are used where a considerable thickness of soft ground overlies a firm stratum, when the upper layer has sufficient consistency to afford a lateral support to the piles, otherwise masonry piers must be adopted.

The piles are usually placed from 2½ to 5 feet apart, center to center, and the distance is occasionally increased to 6½ feet for quays or other works only slightly loaded. Sometimes under abutments or retaining walls the piles are driven obliquely to follow the line of thrust. The Libourne bridge rests on piles 2½ feet apart, and driven about forty feet in sand and silt. At the Voulzie viaduct, on the Paris and Mulhouse railway, some piles were driven eighty feet without reaching solid ground, and the ground between the piles had to be dredged, and replaced by a thick layer of concrete. Piles which have not reached firm ground sustain loads nevertheless, owing to the lateral friction, as, for instance, in the soft clay at La Rochelle and Rochefort piles can support 164 lbs. per square foot of lateral contact, and 123 lbs. in the silt at Lorient. On the Cornwall railway, viaducts were built upon piles, sixty-five to eighty feet long, driven, in groups of four fastened close together, by a four-ton monkey with a small fall. A timber grating is fastened to the top of the piles, or a layer of concrete is deposited, as at Dirschau, Hollandsch Diep, and Dordrecht; or both grating and concrete, as the grating distributes the load and strengthens the piles. Planking is sometimes put on the framing which distributes the pressure, as at London Bridge, but it is considered objectionable as it prevents any connection between the superstructure and the concrete, and increases the chance of sliding. The space between the piles from the river bed to low water is sometimes filled with rubble stones, and sometimes with concrete (Fig. 10), which is less liable to disturbance. When the ground is very soft a filling of clay has been preferred on account of its being lighter than concrete.

Fig. 10.



A mixed system of piling and watertight caissons, of rubble filling and concrete, was adopted at the Vernon bridge. After the piles had been driven the spaces between them were filled up to half the depth of water with rubble stones: a caisson ten feet high was then placed on the top, and a bottom layer of concrete deposited in it. In a month's time the interior of the caisson was pumped dry, the heads of the piles cut

off, and the filling with cement concrete completed to low-water level. The caisson was cut off to the level of the grating as soon as the pier was well above water. The foundation cost altogether £14 8s. per square yard of base of the pier.

The heavy ram of Nasmyth moved by steam, with a small fall, but giving sixty to eighty blows per minute, enabled piles to be driven thirty-three feet in a few minutes, and with much less chance of divergence or jumping than in driving with less powerful engines. In certain soils, in which there is a momentary resistance during pile-driving, it has been proposed to bore holes in which the pile should be afterwards driven.

At St. Louis the annular piles, 3¼ feet in diameter, made of eight pieces of wood, used for guiding the pneumatic caisson, were driven by the aid of the hydraulic sand-pump working inside, the invention of Mr. Eads, M. Inst. C.E.

The load that a pile driven home and secure from lateral flexion can bear may be estimated at from one-tenth to one-eighth of the crushing load, which varies between 5,700 and 8,500 lbs. per square inch. Thus, taking a fair load of 710 lbs. per square inch, a small pile of seven inches diameter will bear about twelve tons, and a pile of eighteen inches diameter will bear about eighty tons, and a pile to bear the load of twenty-five tons used as a unit by M. Perronet should be about ten inches in diameter. According to M. Perronet a pile can support a load of twenty-five tons as soon as it refuses to move more than $\frac{3}{8}$ inch under thirty blows of a monkey, weighing eleven cwts. ninety lbs., falling four feet or under ten blows of the same monkey falling twelve feet. At Neuilly, however, M. Perronet placed a load of fifty-one tons on piles thirteen inches square, but driving the pile till it refused to move more than $\frac{3}{16}$ inch under twenty-five blows of a monkey of the same weight falling 4½ feet; but such a load is unusual. At Bordeaux the driving was stopped when the pile did not go down more than $\frac{3}{16}$ inch under ten blows of a monkey, weighing 1,100 lbs., falling about fifteen feet, but one of the piers settled considerably, the load on a pile being twenty-two tons; whereas at Rouen, by insisting on M. Perronet's rule, no settlement occurred.

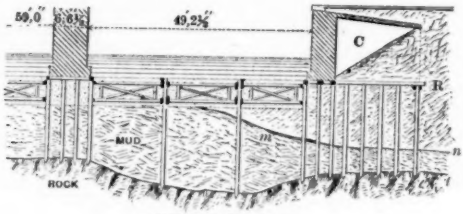
From experiments made at the Orleans viaduct, M. Sazilly concluded that piles might support with security a load of forty tons when they refuse to move more than $1\frac{3}{8}$ inch under ten blows of a monkey weighing fifteen cwts. and falling about thirteen feet.

Various formulæ have been framed for calculating the safe load on piles, which are quoted in a paper by Mr. McAlpine, M. Inst. C.E., on "The Supporting Power of Piles," and in a Paper on "The Dordrecht Railway Bridge," by Sir John Alleyne, Bart., M. Inst. C.E. If Weisbach's formula is applied to M. Perronet's rule it appears that, assuming a safe load, the limiting set of the pile might be 3¼ inches instead of $\frac{3}{8}$ inch for ten blows; and the formula shows that large monkeys should be adopted in preference to a large fall, and in this it agrees with practice for preventing injury to the piles.

In order to provide against the danger of overturning in silty ground, the ground is sometimes first compressed by loading it with an embankment, which is cut away after a few months at those places where foundations are to be built. At the Oust bridge it was even necessary to connect the piers and abutments by a wooden apron, which, for additional security, was surrounded by concrete (Fig. 11). The abutment was made hollow to lighten it, and the embankment, "R," had compressed the silty ground to *m n*. The foundations cost £23 1s. 7d. per superficial yard for depths of from thirty-three to forty-three feet from the natural surface to the rock, or £1 16s. 10d. per cubic yard, a high price due to the difficulties met with and the bad weather. At the bridge of Bouchemaine, near Angers, the bending of the piles, which traverse about twenty feet of silt, was stopped by surrounding them with great masses of rubble stones.

Occasionally foundations on piles have failed or suffered great sets or lateral displacements. At the Tours bridge, many arches have fallen at successive times; holes in the foundations had to be refilled with lime, and below certain arches a general bed of concrete was afterwards established.

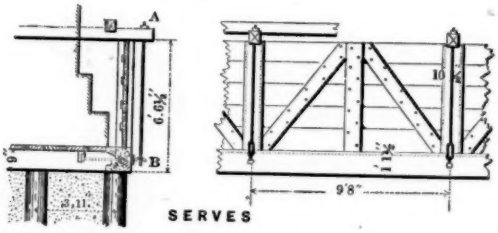
Fig. 11.



OUST VIADUCT

Floating caissons require a bottom carefully levelled on which to be lowered. Labelye, in 1750, deposited the caissons of old Westminster bridge on the dredged bottom of the river; but usually this kind of caisson is deposited on piles cut off to one level. These caissons have oak bottoms and movable sides of fir, and enable the masonry piers built inside to be lowered on piles previously driven. The oak bottom serves as a platform for the pier, and the movable fir sides can be used again for other caissons. At Ivry, with only two sets of movable sides, the contractor was able to put four caissons in place in one month. The bottom, which consists of a single or double platform, has timbers projecting underneath which fit on to the rows of piles. The movable sides are sometimes made in panels which fit into groves both in the bottom framing and in upright posts, placed about ten feet apart, which are tenoned at the bottom, and kept in place at the top by transoms going across the caisson. The different parts of the sides are tightly pressed together by the bolt, A B (Fig. 12). In other instances, as at the bridge of Val Benoit over the Meuse, the sides

Fig 12.



SERVES

butt against the vertical sides of the bottom, against which they are pressed by keyed bolts, D, placed at intervals of five feet (Fig. 13). The caisson is kept near the shore whilst the first courses of masonry are being built in it; it is then on a favorable opportunity floated over the site of the pier prepared to receive it, and is gradually sunk by letting in water.

At the Bordeaux bridge the caissons had a height of twenty-six feet, and were divided in cases by transverse rods. This work, which comprises seventeen arches, was founded in a great depth of water, about the year 1820, by the engineers Deschamps and Billandel.

In sea works the laying of foundations in the water is managed differently. Thus artificial blocks of concrete may be deposited by the help of divers, as at Dover pier; or much larger masses may be moved by powerful machinery, as, for instance, blocks of 150 to 200 tons put down at Brest in 1868, and at Dublin by Mr. Stoney, M. Inst. C.E. For small landing piers, and for piers of bridges in rivers not exposed to the breaking up of ice, artificial blocks or metallic frameworks may be placed under water on the top of timber piles cut off level, a plan adopted by Mr. Maynard, M. Inst. C.E., on a foundation of screw piles.

Screw piles were introduced by Mr. Mitchell, M. Inst. C.E., for securing buoys. They can be applied with ad-

vantage to the construction of bollards and beacons, on account of the resistance they offer to drawing out; but as in the process of screwing down the ground is more or less lossened, judgment must be used in employing them for mooring or warping buoys. In foundations for beacons, they should be screwed down from fifteen to twenty feet below the level to which the shifting sand is liable to be lowered. Even when all cohesion of the ground is destroyed in screwing down a pile, a conical mass, with its apex at the bottom of the pile and its base at the surface, would have to be lifted to draw the pile out. The resistance to settlement is also increased by the bearing surface of the screw; and the screw pile is accordingly to be preferred to an ordinary pile in soft strata of indefinite depth, or when the shocks produced by ordinary pile-driving are liable to produce a disturbance. The screw pile has likewise the advantage of being easily taken up.

Screw piles have been principally used

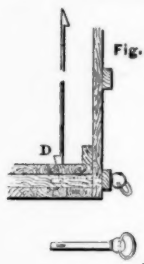


Fig. 13.

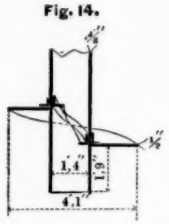


Fig. 14.

VAL BENOIT

in England and in the United States. They have usually one or two spirals projecting considerably from the shaft, these spirals being cylindrical for soft ground and conical for hard ground, and either of wrought iron or of cast iron. The shaft may be of wood or, by preference, of iron, which must be pointed at the end for hard ground, but cylindrical and hollow when the ground is soft. The screw will penetrate most soils except hard rock; it can get a short way into compact marl, through loose pebbles and stones, and even enter coral reefs. A screw pile turned by eight capstan bars, twenty feet long, each moved by four or five men, with a screw four feet in diameter, passed in less than two hours through a stratum of sand and clay more than twenty feet thick, the surface of which was about twenty feet below water, and dug itself to a depth of about one foot into an underlying schistous rock. At the Clevedon pier screw piles penetrated hard red clay to depths varying between seven and seventeen feet, and although the screw had a pitch of five inches they rarely went down more than three inches in one turn. Mr. W. Lloyd, M. Inst C.E., has recorded an unsuccessful use of screw piles, which in the shifting sandy bed of a South American river became twisted like a corkscrew, and were overturned in the first breaking up of the ice. At Hamburg screw piles, in sets of three and joined at the top, are used as bollards. The piles are hollow wrought-iron tubes, $\frac{3}{8}$ inch thick, furnished with a screw both inside and out, with a pitch of one foot (Fig. 14). To screw them down two capstans were used to pull the two ends of a rope wound round the head of the pile, the force transmitted to the pile being thirty times that applied at an arm of the capstan, and towards the close, when the pile had been forced down nearly thirteen feet, seven men were required to work each capstan. At the commencement each turn of the screw produced a descent of ten inches, and hardly nine inches at the end. A vessel struck, in 1852, against one of these bollards, and broke off the top without shifting the piles.

Piles with discs, used in the first instance at the Leven and Kent viaducts, by Mr. Brunlees, M. Inst. C.E., differ little from screw piles except in the method of sinking them. This operation was performed by sending a jet of water down a wrought-iron tube inside the cast-iron pile, which washed away the silty sand from underneath the disc

and caused the pile to descend. The sinking cost about 2s. 6d. per lineal foot, whereas at Southport pier, where water was obtained from the waterworks, and ten piles were sunk per tide, the sinking only cost 4½d. per lineal foot. Wooden piles, with a cast-iron shoe carrying a disc, might easily be sunk in the same manner, the water pipe being carried eccentrically through the disc.

Hollow wrought-iron piles have also been forced down by blows of a monkey, in silty and sandy ground interspersed with boulders, to a depth of about sixty feet; the thickness of the piles being about one-nine inch, and the diameter 19⅜ inches. On the Cambrian railway, Mr. Conybeare, M. Inst. C.E., drove wooden piles down below the surface, by means of a lengthening piece of cast iron on the top, a piece of wood or lead being interposed between the monkey and the cast iron.

Large masonry piers carried through thick layers of soft ground to a solid bed may be constructed by various methods, and constitute the best kind of foundation in such a situation.

The method of cased wells is suitable where the silt is sufficiently compact and watertight to admit of pumping the well dry, and where the depth of water is small and can easily be kept out by a cofferdam or a caisson without a bottom. The well is sunk by the ordinary methods of sinking wells or driving headings in silty ground. At the Auray viaduct, a muddy stratum, twenty-six feet thick, was got through by this method. In building the abutment of a bridge over the Vilaine, in Brittany, resting on six pillars carried down fifty feet below the water-level, the same method was adopted; but for the lower portion of the excavation a smaller framing had to be sunk inside. A pillar fifty feet deep requires about twenty days for the excavation, and twelve days for building the masonry. The cost is from £1 16s. 10d. to £2 9s. per cubic yard of foundation complete. When the pier is so wide as to render the strutting difficult, an outer ring can be first lowered, which serves afterwards as a casing for excavating the inner portion. Where permeable gravel or very liquid silt has to be traversed it is necessary to resort to tubular foundations.

SOLID STEEL CASTINGS.

By M. FERDINAND GAUTIER, of Paris.

Journal of the Iron and Steel Institute.

When steel is cast in an iron ingot mould, or a mould of any kind, usually the metal after cooling is not entirely sound. Cavities of a more or less rounded shape are seen inside, apparently caused by a gas escaping from the mass. Mr. Henry Bessemer, the first among metallurgists, has demonstrated that these blow-holes were filled with oxide of carbon, and this view has since been entirely confirmed.

Refining produces oxide of iron, Fe_2O_3, which reacts on the carbon according to the formula:

$$Fe^3O^4 + C = 3FeO + CO.$$

The protoxide of iron passes into the slag, and the oxide of carbon remains in dissolution within the metal. But the solubility of the oxide of carbon diminishes rapidly during the cooling period, and some gas bubbles remain imprisoned in the solidified mass.

When these blow-holes are altogether inside, and do not burst through the crust, they remain silvery white; it is a simple solution of continuity, and to get rid of them it is sufficient to weld the metallic parts by reheating and the use of a hammer or rolls. What becomes of the carbonic oxide? Is it re-incorporated with the metal? This has not been determined yet. But the fact is that when rolled steel bars, drawn from honey-combed ingots, are broken, no trace is found of this kind of defect.

When the blow-holes communicate with the outside, and the sides of the

ingots are pierced with small holes, well known to the steel manufacturers, the color is no longer a silvery white; they assume more or less the colors of the rainbow, and even become black. This takes place because the carbonic oxide of the blow-holes is transformed by the contact with the air into carbonic acid, and oxidation of the metal occurs.

$$3CO^2 + {}_3Fe = Fe^3O^3 + 3CO$$
$$\text{or, } 4CO^2 + {}_3Fe = Fe^3O^4 + 4CO$$

according to the temperature when the reaction takes place.

We know, from Mr. I. L. Bell's works, that the oxidation of iron by carbonic acid begins at about 300° C. (570 F.), and is very strong at a high temperature.

Hammering and rolling do not entirely eliminate these blow-holes, as a perfect welding of the metallic particles is prevented by the presence of oxide of iron. There remain some black streaks, which sometimes penetrate to a depth of $\frac{1}{10}$ inch (2 millim). In order to correct these surface defects, the welding must be done by allowing the piece to undergo heating at as high a temperature as possible; the steel is there covered with sand, and hammered vigorously. The oxide of iron which prevented welding is combined with silica, and forms a silicate which pressure easily expels. This is a general practice in the manufacture of fine steels.

It will be seen that it is an easy matter to remove blow-holes when the steel has to undergo mechanical elaboration; but it is not so with castings, and it is very important to prevent the formation of these blow-holes when sound pieces are wanted, and pieces the resistance of which can be relied upon.

All steels have not the same tendency to produce blow-holes while solidifying. Generally, the more a steel is carbonized, the better it will flow while casting, and fewer blow-holes will form. On the contrary, the more a steel is decarbonized, the less fluid it will be, and more blow-holes will form. It is probable that hard steels keep better their oxide of carbon in dissolution, or else it escapes more freely on account of the greater fluidity of the metal.

This property of highly carburized steels has led to the casting of thin pieces without blow-holes. A very hard steel, with about 1.5 per cent of carbon, is cast in thoroughly dry moulds. The castings thus obtained are left for several days in an oxidizing atmosphere, or even in contact with a bed of oxide of iron or zinc, as in the manufacture of malleable iron. Very strong products are obtained in this manner, but they must be very thin, and even then they are far from being without blow-holes. As is the case with malleable iron, thick pieces are out of the question, as decarburizing goes little by little, from molecule to molecule, and through a solid body. If the pieces were too thick, the outside would become oxidized before there was a thorough decarburization of the mass. This very interesting process is carried on with success at Sheffield.

We have to speak now of the German manufacture of steel without blow-holes. Every one will remember those splendid ingots, clean-fractured, growing in weight at each successive exhibition, first beginning with two tons, and finally reaching 45. The Krupp ingots, and the cast wheels and bells of Bochum, certainly astonished the metallurgical world for some time. The process of manufacture was kept a most profound secret, and has not yet been published.

More than six years ago, the Terre-Noire Steel Works found out, by reasoning rather than by practice, the process of the German works, and the improvements they have made have radically transformed the result.

It seems well proven now that the German products without blow-holes are obtained by an addition of a very siliceous pig just before casting; and thus they are found to be highly carburized, and the chemical analysis shows a rather considerable portion of silicon. To find an explanation of the result, we must go back to the theory of the Bessemer process. It is well known that the combustion of the silicon takes place during the first part of the operation; there is no flame, or rather, there is a series of brilliant sparks; the yellow sodium band, characteristic of all flames, is not seen; no oxide of carbon is formed while there remains any silicon to be oxidized; the silicon decomposes the oxide of carbon, or, which is the same, prevents it from forming; therefore, if

blow-holes are filled with oxide of carbon, they will be made to disappear by the addition of silicon

$$Si + 3CO = SiO^2 + 3C.$$

The carbon is deposited and dissolved in the steel, and silica is formed. Experience shows that steels treated in this manner are generally without blow-holes.

In order to prove this theory, the following experiment can be made. A bath of siliceous pig is formed in a Siemens-Martin furnace, and refined by successive additions of iron or steel. Tests are taken at short intervals. At first, these tests are quite sound, but they become full of blow-holes. If a chemical analysis is made, it is found that the first honey-combed test contains no silicon, while the preceding tests contained some.

If the products obtained in this manner are without blow-holes, the quality is rather inferior, in spite of the long annealing they usually undergo after casting. We will analyse the several causes of this inferiority.

1st. These steels are very highly carburized: the pig being generally poor in silicon, the operator is led to charge a rather large quantity so as to be sure that it will act effectually.

2d. The silica produced by the chemical reaction that destroys the blow-holes is, indeed, generally combined with oxide of iron in the bath; but the slag is slightly fluid, and remains in the steel, which is thus made pasty. It lessens its strength; it may even make it crumble when trying to work it hot.

3d. There remains in the final product an excess of silicon, which, in addition to the excess of carbon, tends to lower its quality.

This influence of silicon on pigs and steels has been anything but clear for a long time, and it may be said that even now it is not perfectly known. I think it useful, at any rate, to speak here of the curious experiments made by the late Wenzel Mrazek, of the School of Mines at Przibram (Bohemia).

Silicium always had the very worst reputation in metallurgy. Karsten, whom the Germans modestly call the immortal, was the first to assert that silicon gave to iron a special red-short quality (faul-brüchig) with a peculiar fracture—an earthy fracture. This opinion was allowed to spread without any disputing until lately, when Mrazek showed that the action attributed to silicon should be ascribed to the silica found in the silicate mixed with the iron; for if metallic silicon of good quality is added to a certain quantity of iron, the original qualities of the metal are in no point changed. This view is much more logical from a chemical standpoint; for it is well-known that in the refining period silicon disappears first of all; it would be necessary to suppose that after refining is done the silicon is again reduced in spite of the oxidizing influences, passes back into the metal, and makes it red short. It is, therefore, impossible to believe there is any free silicon in the iron in a notable quantity; but the material found is silicate, such as that of steel without blow-holes of which we have just spoken.

But if silicon is harmless in a soft metal, it is far from being so in a product containing carbon. In a preceding paper, I spoke of the incompatibility of carbon and phosphorus in steel; and of the necessity of suppressing carbon when using phosphorus. Mrazek's experiments prove that there is also incompatibility between carbon and silicon in steel. The simultaneous presence of carbon and silicon causes brittleness, hot or cold, while a cast metal, containing slight traces of carbon only, will bear up to 7 per cent. of silicon, and still roll well at red or white heat, and will even weld perfectly well. Therefore, if a quantity of silicon is wanted in a cast steel (and there must be some to ensure perfect soundness) it will be necessary, in order to obtain a good product, not to entirely suppress the carbon, but at least to lessen the quantity considerably.

At the Terre-Noire Steel Works, the manufacture of steel without blow-holes has been perfected by using a silicide of manganese and iron, which gives to the product remarkable qualities. The silicon prevents blow-holes by decomposing the oxide of carbon, which is in dissolution, and tends to escape during solidification. The manganese reduces the oxide of iron, and prevents a further production of gases by the reaction of the oxide on the carbon. We have seen

above that in the decomposition of oxide of carbon by silicon, silica was produced, and afterwards a silicate of iron, which remained interposed within the steel. The manganese allows the formation of a silicate of iron and manganese, which is much more fusible and passes into the slag. For this reason, the metal is not altered by a foreign body, and this is a capital point.

In order to show plainly the complete structural difference between steels without blow-holes obtained with silicon alone and those obtained with an alloy of silicon and manganese, Mr. Pourcel operates in the following manner. In a porcelain tube he places two receptacles, one holding steel by silicon alone, and the other steel by an alloy of silicon and manganese. A current of chlorine is passed until all the iron is removed in a state of chloride. It is seen then that in the first receptacle, there remains a net work of silicate of iron preserving the original form of the pieces, while the steel by alloy leaves no residuum.

We already know that an excess of manganese remaining in a steel improved the quality of it; we learn from Mrazek's experiments that a slightly carburized steel containing free silicon and manganese at the same time, can be excellent. A steel bearing

Silicon	1.50
Manganese	0.76
Carbon	0.18

is very strong and rolls perfectly. It is therefore useless to be much concerned about the excess of manganese or silicon that may remain in the steel cast in this manner.

More than 500 charges of steel without blow-holes were analysed and tested at Terre-Noire, and below are the results obtained in tensile strength with cast metal of three different qualities :

CHARGE IN TONS (2,240 lbs.) PER SQUARE INCH.

ELONGATION MEASURED ON A FOUR-INCH TEST BAR.

		Raw Metal, coming out of the Mould. Tons per Square Inch.			Metal, reheated and cooled slowly. Tons per Square Inch.		
	No.	Elastic Limit.	Breaking Load.	Elongati'n per Ct.	Elastic Limit.	Breaking Load.	Elongati'n per Ct.
Hard Steel for Projectiles.	948...	21.0	40.4	1.7	24.8	52.0	7.25
	966...	19.8	39.0	2.0	22.4	51.0	8.50
	1,355...	22.0	34.0	2.0	23.6	51.5	8.00
	1,502...	24.3	33.2	1.0	27.3	57.1	7.40
	1,737...	21.2	35.0	1.7	24.2	59.0	7.00
	1,872...	22.0	38.9	1.5	25.6	56.0	3.40
Strong Soft Steel.	1,545...	23.2	40.5	2.0	24.2	52.0	12.2
	1,558...	21.7	43.7	3.3	25.3	51.0	14.0
	1,563...	20.6	41.8	2.5	24.3	51.9	17.5
Very Soft Steel.	2,078...	10.3	32.7	12.5	16.0	34.5	28.5
	2,081...	12.7	34.4	12.4	18.8	35.6	25.6
	2,149...	13.3	33.2	12.5	18.0	35.4	24.3

This metal is in no respect similar to the steel without blow-hole castings made until now; it can be hammered, and rolls without flaws; it resists hard blows, and when soft will stand considerable bending before breaking; in a word, in the form of a moulded piece, it possesses the same properties as a rolled and hammered metal.

This brings us to speak of a very remarkable paper by M. Chernoff, an engineer at the Aboukoff Steel Works, St. Petersburgh. This paper is on the structure of steel. A steel ingot is a

body coming more or less rapidly from a liquid to a solid state, and preserving a more or less crystalline texture. There is one general rule, and that is, that all metals possessing a crystalline texture are brittle, as for instance antimony, bismuth, zinc in ingots; while a confused and irregular texture corresponds to the greatest resistance. Steel is not an exception to this rule, and if a fracture is crystalline, the piece is very brittle. Fortunately, this defect can be modified in several ways.

1. By simple re-heating to cherry red, when an ingot with coarse fracture, and completely lacking the reqnisite strength, will suddenly be transformed after ordinary cooling into a fine grained and strong product.

2. By hammering at a sufficiently high temperature cast steel loses its crystalline structure, provided this hammering is continued, while cooling takes place, down to a certain point varying with the different steels, and beyond which the metal preserves its whole power of resistance. If hammering was left off at a high temperature, and the metal abandoned to itself, the crystalline structure would re-appear, and with it the lack of strength.

3. The rapid cooling of a cast metal also destroys the crystalline state: this was observed in wrought iron armor plates.

Steel, then, seems to behave like an exceedingly concentrated dissolution of salt easily crystallizable, the crystallization of which is prevented by certain precautions only.

For Mr. Chernoff, as well as for us, unwrought cast steel is neither softer nor weaker than a steel of the same grain forged at a suitable temperature.

Of course, we are speaking of steels without any defects, cracks, or blow-holes, or else a comparison would no longer be possible.

Mr. Chernoff took a coarse-grained cast steel ingot and had it cut lengthwise into four parts. One of the quarters was transformed directly, on a lathe, into a test bar. The second was heated to a bright red, forged under a steam hammer, the forging being stopped whilst the piece was yet rather hot. The third piece was heated up to the point at which the hammering of the preceding piece had been left off, and then was allowed to cool freely. The fracture showed a very fine grain, very similar to that of the forged piece. These two quarters were also transformed into test bars.

The results of the traction tests were as follows:

In tons per square inch.	Breaking Load.	Elongation.
No. 1. Untouched ingot.....	38.5	2.3
No. 2. Forged ingot.....	45.5	5.3
No. 3. Ingot reheated and cooled in air.....	43.0	16.6

We believe, like Mr. Chernoff, that it is possible to have a steel cast directly just as strong as if it was hammered, provided that the metal be regularly without blow-holes, and crystallization done away with.

On Mr. Chernoff's part, this is a theoretical intuition, resulting from an enlightened study of a material. With us it is more than an opinion, for we bring a practical realization.

The very interesting fact that cast metal may have a higher density than forged and rolled metal has not yet been pointed out, yet at Terre-Noire it was ascertained that this density varied from 7.8 to 7.9, while that of rolled steel never went beyond 7.81. It would seem, from this experiment, that the rolling process causes not only a different arrangement of the molecules in reference to one another, but also a slight increase of volume. The question is now, in presence of these two facts—1st. that a cast steel may be more dense than a hammered one; 2nd. that its strength may also be superior. What can be the use of the compression of liquid steel?

Compression applied to steel was tried simultaneously, 12 years ago, at Terre-Noire and at Manchester by Sir Joseph Whitworth. The first results obtained on the Continent were such as to cause the process to be soon abandoned and forgotten. Two years ago new experiments were made in many places; but it seems well proved now that there is nothing to be done in this direction.

At Manchester, on the other hand, it is said that very interesting results were obtained: the compressed metal would bear 43 tons per square inch, with 32 per cent. elongation on a 2-inch length (which corresponds to 20—22 per cent.

on 4 inches). How are we to explain this difference? Undoubtedly the *modus operandi* is not the same, for in France and in Prussia no such result was ever reached.

It is to be regretted that, two years ago, during the visit paid at the Manchester works, the members of the Iron and Steel Institute were not enabled to get any idea of the efficiency of the process, otherwise than by a walk in a deserted workshop; the explanation of this great difference of results between two neighboring countries applying the same process could then have been found.

Compression cannot seriously modify density, for when the cast metal is perfectly sound, it possesses a density higher than that of forged metal. But it might very well happen, that, at the time of solidification, crystallization is stopped by a powerful compression, and the metal acquires the properties, similar to those of forged product. But at any rate, if compression can, in skilful hands, give good results, these results can be obtained in another way, a simpler one, and I believe a surer one; I mean the process I have just explained, in the name of the Terre-Noire Company. The first practical application of the metal without blow-holes, now manufactured at Terre-Noire, is due to the genius and perseverance of Mr. Euverte, the skillful director.

The use of hollow projectiles bursting inside a hip, after having penetrated through the armor, has, with an equal power of penetration, great advantages on solid projectiles. They bust into pieces both numerous and dangerous; they shake, dislocate, and break the inside lining to a great extent, and the resulting leakages cannot be stopped up.

Chilled cast iron balls being low priced compared with forged steel ones (six or eight times cheaper), and the manufacture of them much more rapid, a great economy could be realised, from the military stand point, by using them.

Gradatz, Finsponge, Terre-Noire, Châtillon, Piombino, and Palliser, furnished samples, but without any good results; the projectiles split and break in normal firing, when the plates they strike against are rather thick.

In oblique firing, even against comparatively thin walls, the chilled iron balls break at 20 deg., and at 30 deg. they are pulverized.

This deformation of the projectile diminishes its penetrating power, since the *force vive* is spent in sterile efforts to break, and, according to Capt. Noble's formula:

$$\frac{WV^2}{2g} = \pi RKB^2$$

it is this *force vive* that measures the penetrating power with equal diameters. Furthermore, the cracks, which at first sight do not seem very hurtful, prevent a sufficient tension of the gas during explosion of the powder, and poor effects are obtained.

In spite of the seeming boldness in substituting cast for forged steel, in spite of the little faith of the ordnance men themselves, at this moment the Steel Works at Terre-Noire manufacture regularly 9½ inches projectiles capable of penetrating, at an angle of 30 degs., plates eight inches thick, with the relatively slow speed of 1,400 feet per second. The projectiles never break; some times the point is slightly bent. This is, I venture to say, the most severe test to which a cast metal can be subjected, for in the trials of which I speak, this cast metal showed itself superior to forged steel both in strength and regularity.

After this undisputed success of which you undoubtedly have heard, for it is now nearly a year old, Mr. Euverte asked himself the question whether the strains to which the metal is subjected in a cannon are not much weaker than when a projectile pierces an armor plate? It is true there is a difference, for in one case the metal is compressed and in the other it is drawn.

It is not known yet whether it will be possible to begin immediately the manufacture of cannons in one piece as was formerly done in cast-iron cannons for the navy and the defence of towns. But there is one idea which presented itself naturally, and is being studied even now, —it is nothing less than the replacing of the cast-iron body by a cast-steel one in coiled guns. Thus, a metal hardly able to stand a strain equal to ten tons, will be replaced by another requiring thirty-three tons at least to break it, supposing, for security's sake, that the softest metal was used.

Will the application of this new metal stop here? Decidedly not. You know that in the latest trials at Spezzia, the 100-ton gun was not able to force a projectile through a forged steel armor plate. It is but just to say that the plate was pulverized; but for some ordnance men this result is very interesting; they consider a ship is sufficiently protected when the outside plating is not pierced through by the projectile; they would even prefer to use plates thick enough to absorb the *force vive* of the ball while being pulverized, even if they should leave a portion of the ship bare, for it seems very improbable that the same portion of the ship could be struck twice, considering the distances at which naval fights will take place with the new artillery. If the new cast metal is used for this purpose, the softest qualities only must be used, but so far, every circumstance points to a great result to be reached with enormous economy.

As to the different industrial applications, they naturally present themselves to every mind, and we might even say they will push themselves forward, bearing with them these two great advantages—solidity and economy.

THE MAGNETIZATION OF STEEL TUBES.

By M. GAUGAIN.

Translated from "Revue Industrielle."

If a steel rod, neutral as regards magnetism, be introduced into a magnetized steel tube, and after a moment withdrawn, it will be found to be feebly magnetized in the same direction as the tube. But if while the rod is in the tube, both tube and rod be heated to about 300°C, and then left to cool, it will be found upon withdrawing the rod that the magnetism of the tube is much reduced, and the polarity of the rod is reversed.

I suppose that at the ordinary temperature, the rod assumes a polarity opposed to that of the tube during the time that it is within the tube, but upon being withdrawn friction is inevitably produced, and this reverses the polarity. It would seem that at the elevated temperature we have on the one hand the *inverse* magnetism of the rod, induced by the tube considerably augmented by the heat; and, on the other hand, the *direct* magnetization developed by the separation of rod and tube much weakened, the tube having lost much of its magnetism by the separation. The inverse charge therefore remains predominant.

Analogous efforts are produced when a magnetic rod is introduced into a tube that is not magnetic. At the ordinary temperature the tube upon being separated from the rod will be found magnetized in the same direction, but if heated while together and allowed to cool before separating, the tube will be found to contain a charge the reverse of that of the rod.

It is not necessary in applying the heat to produce an inverse charge of magnetism, that either rod or tube should be in a neutral state. It is sufficient that they be unequally magnetized and in the same way; then if there is enough difference, the one with the weaker charge will be found with its poles reversed after separating them.

Now considering the case of a tube and rod in the neutral condition magnetized by Elias's process,—If the charge is imparted at the ordinary temperature, the two when separated are found magnetized in the same direction. This fact has been verified by M. Jamin.

The same result is obtained when the system is magnetized while at a temperature of 300° or 400°, if the rod and tube are separated immediately. But having charged the system at a high temperature and then allowed it to cool, the rod and tube being separated, exhibit opposite polarity; it is only in a particular case that their magnetism is in the same direction; the polarity varying with the bore of the tube, the coercitive quality of the steel, and the intensity of the current that charges them.

I have experimented with tubes of $\frac{1}{4}$,

½, ¾, and 1 millimeter diameter; the exterior diameter of each was 10 and the length was 300 millimeters.

Rods and tubes were made of the steel known as *doux Petin-Godet.*

The following table exhibits the result of experiments with the tube of ½ millimeter bore.

I	M	M′	*m*	*m′*
3.4	+ 4.6	+ 8.0	− 1.5	+ 5.0
7.5	+ 20.0	+ 22.2	+ 2.5	+ 2.5
14.5	+ 56.0	+ 21.0	+ 17.2	− 15.0
20.0	+ 80.0	+ 27.0	+ 28.2	− 25.1
29.0	+ 80.0	+ 30 2	+ 28.0	− 28.0
38.0	+ 87.2	+ 34.0	+ 24.0	− 29.5

The numbers in column I give the intensities of the currents employed.

Columns M and M′ show the force of magnetism in the rod and tube respectively, when measured directly after charging, and before cooling.

Columns *m* and *m′* indicate the magnetic charges in rod and tube after cooling.

The + sign signifies the direction of the magnetism afforded by the current to either rod or tube at the ordinary temperature. The − sign indicates the reverse polarity.

An inspection of this table shows that the magnetism *m* of the rod which is *inverse* for the feeble current, 3.4, becomes *direct* when the intensity of the current augments; and that on the contrary, the polarity of the tube *m′* which is *direct* for the currents whose intensities are 3.4 and 7.5, becomes *reversed* with stronger currents.

These results may be regarded as the consequence of a simple fact stated in the beginning of this note. It appears from the above table that before cooling the system, the rod and tube are magnetized in the same way; that the initial charge M′ of the tube overcomes that of the rod when the charge is feeble, and on the contrary, the charge of the rod surpasses and overcomes that of the tube, when the intensity of the current passes a certain limit. Accordingly it is the magnetism of the rod which should be inverted during cooling in case of feeble currents, and the polarity of the tube which should suffer change in the case of more energetic currents.

The differences exhibited by the columns M and M′ for the different intensities point to the principle established by M. Jamin, that the current penetrates to greater or less depth according as it is more or less energetic.

The experiments with the tubes of ¼, ¾ and 1 millimeter, gave results similar to those detailed above.

REPORTS OF ENGINEERING SOCIETIES.

At a meeting in November, of the Liverpool Engineering Society, Mr. John S. Brodie, member, read a paper on "The Application of Blast Furnace Slag to the purpose of road-making." To begin with, Mr. Brodie explained that when soft stone is for "bottoming" it gradually works through the harder surface and has to bear the traffic in its place, for which purpose it was never intended. Consequently the road is rendered uneven and requires much mending. It was claimed for good blast furnace slag that when the bottoming and surface layers are both formed with it, the result is a durable roadway of uniform hardness throughout, and could be obtained at a less cost than the ordinary Mc Adam system. The mode of preparing the foundation for a road, and the subsequent formation of the successive layers of slag was next gone into, details of curvature and other matters being illustrated by drawings. It was shown that no foreign matter should be allowed among the slag, as it would diminish the durability of the roadway. The best slag for road making was stated to be that produced in smelting the Cleveland ore into gray or foundry iron. In conclusion prices of forming slag roads were given.—*Engineering.*

The Institution of Civil Engineers.—At the opening meeeting of the present session of this society, a paper was read on "The Progress of Steam Shipping during the last Quarter of a Century," by Mr. Alfred Holt, M. Inst. C. E., of Liverpool. With a view to direct the attention of members to the obligation voluntarily incurred, of doing all in their power to further the interests of the society by presenting good papers, the council have issued, under the title of "List of Subjects for Papers," a tractate which gives some interesting information respecting the funds under their control for the award of premiums. From this it appears that between £400 and £500 is available annually for rewarding the authors of "approved original communications." The awards take the shape of Telford and Watt Medals (which are the most highly prized), instruments, and richly bound books. There will also be adjudged this year the quinquennial Howard prize of the value of about £80 "to the author of a treatise on any of the uses or properties of iron, or to the inventor of some new and valuable process relating thereto," such author or inventor being a member or an associate of the Institution. This manifesto will doubtless have due effect in stimulating the members to uphold the high reputation the Institution has acquired as a disseminator of professional knowledge, and will it is hoped result in the

council being enabled to continue the publication of four volumes of "Minutes of Proceedings" annually.

KING'S COLLEGE ENGINEERING SOCIETY.—At a recent meeting of this society, a discussion was held on the several appliances for the pavements of roads. The discussion was opened by Mr. Guinness, who gave a general description of the macadamised road and the steam roller. Mr. Alliman gave some useful information in reference to the asphalte and wood pavements, and described the construction of roads in Russia. Mr. Samuel described the ancient Roman roads, and suggested that a macadamised road, consisting of broken granite and pitch, used in the same manner as the granite, would be very useful in places where the traffic was not too great. Professor Tennant, in summing up, said that the subject was of great importance, especially where so vast a population as that of London was concerned. He first referred to the alterations in transit which had resulted from the introduction of the locomotive, and as an illustration of the progress made, he stated that when he first came to London it took him thirty hours and cost him 30s. to travel the distance, which now occupied only four hours, at a cost of 10s. The Strand at that time was paved with boulder stone, similar to the present pavement of Scotland yard. Wood pavement was introduced about thirty years ago, but was soon discontinued on account of the exhalations which arose from it when saturated, and which tended to produce fever. The Professor then proceeded to examine the advantages and disadvantages of wood pavement. He said that four years was the longest time that it has been found to last in London, and that it was twice or three times as expensive as granite. The expense of the present granite pavement of the Strand was 17s. 6d. per square yard, but, formerly, when large stones were used, it only cost 15s. per square yard. It was found that horses could not get a thorough foothold on the larger stones, and resort was consequently had to the present material. Amongst the disadvantages which had presented themselves since the introduction of wood pavement was the increase in the number of accidents, which had occurred chiefly on the noiseless approach of Hanson cabs and other vehicles.

LIVERPOOL ENGINEERING SOCIETY.—At the first of the October meetings, Mr. J. E. Claudy, B.A.B.E., late sub-resident engineer on the San Paulo and Rio de Janeiro Railway, read a paper on "Railway Engineering in Brazil." He commenced by pointing out that in foreign countries an engineer's energies are not cramped by musty Acts of Parliament, local boards, and the red-tapeism too common in professional practice in this country. Abroad an engineer may delve and dig, hack and hew, with nature alone to oppose him. In carrying out a Brazilian railway an engineer has to look sharply after contractors and often do the work of an inspector or foreman. He ought, therefore, to have a good knowledge of the bricks, mortar, stones, and all materials used in construction, as well as to be able to discriminate between good and bad work. He pointed out that most of the lime used for mortar was made by burning oyster shells, limestones being very scarce, although there was a brittle stone called "saibre" much resembling it which was often used if contractors were not looked after. Brazil has an area of three and a-quarter millions square miles, and only 1200 miles of railway working, whilst the United States, with a less area, has no less than 78,000 miles, and at the present time railway enterprize in Brazil is quite at a standstill, as there are no good roads or means of communication to act as feeders. Horses and mules are about the safest means of traveling. As an instance of mountain railways, the author called attention to the San Paulo Railway, which, in five miles rises from the sea level at Santos, to St. Paulo, 2600 feet above. This railway is worked on the wire rope system, the above distance being divided into four stages or "lifts," at each of which is situate a stationary engine. The heavy rainfall in Brazil, as in most tropical climates, has to be guarded against by allowing ample water ways all along the line of works—thirty inches in twenty four hours is not exceptional. He then went on to deal with the various engineering operations necessary in surveying, laying out, and constructing the railway, and pointed out that the lump sum contract system is seldom followed, the work being let and measured on a schedule of prices previously agreed upon. He concluded by pointing out that an engineer's position on a foreign railway is not a bed of roses, he having to undergo many hardships. The meeting was well attended, and among those present were noticed several Brazilian gentlemen.

IRON AND STEEL NOTES.

THE New Zealand Titanic Steel and Iron Company have sent over some of their pig iron made from the metallic sands which lie along the shores of New Plymouth in Taranki. The company have two furnaces there, and cast their pigs 78 pounds each. The iron, which came consigned to a merchant firm in Wolverhampton, was tested for puddling purposes at the Shelton Bar Iron Company's Works, Stoke upon Trent. It is found that the waste was as much as 20 per cent. since it required 1 ton 3 cwt. 3 qr. and 14 pounds of pig to produce a ton of puddled bars. The quality however was declared to be excellent in both a cold and hot state, whilst it bore a tensile test of 1 to 1¼ above the 22 tons per square inch, Admiralty test, for best bar iron. At Taranki the price of the pigs is about £3 10s. per ton. At that quotation the iron ought to secure for itself a market in Australia, where, just now a better business is being done in English pig iron than for many months past.

PHOSHHOR STEEL.—Without dwelling again on the nature and properties of the so-called phosphor steel, we may be allowed to remind our readers that the production of this

metal is still going on, showing it has been found possible to manufacture so-called steel rails containing as high as 3 to 5-thousandths of phosphorus, when, in the olden time, it was believed that 0.5-thousandth was too high a percentage. At the same time the steel thus produced rolls quite freely, and stands most ordinary tests remarkably well. The advantages of this metal are that it permits of the use of lower grade and cheaper ores, and the re-employment of old rails. These contain on an average 6 thousandths of phosphorus, but by a careful admixture with a pure pig iron a medium can be obtained which produces satisfactory results of a saleable character, the cost price of which is stated to be lower than that of the ordinary Bessemer.

MALLEABLE STEEL.—For some time past the manufacture of homogeneous or malleable cast steel has been attempted in a different direction, namely, by the compression of the molten metal. Sir Joseph Whitworth seems to be practicing this as a regular operation at his works in Manchester, but, from what scanty information we have gleaned as regards his process, it appears to consist rather in a mode of hydraulic forging than of compression proper of a liquid mass. Under a very severe pressure, steel is instantaneously solidified, and a solid, not a fluid, is then acted on by the compressing apparatus. We may add that Mr. Whitworth employs nothing but steel of the best quality, and smelted in the crucible. A French engineer, named Bouniard, late foreman at Terre-Noire, perseveringly tried the compression of fluid steel, and his plan was experimented with on a large scale at Revallier, Bietrix and Co.'s, at Saint Etienne, as well as at Terre Noire. No practical results were obtained.

Until very lately, erroneous ideas existed regarding the effects of compression on fluid metals, the density of which was supposed to be increased by such a process. Mr. Euverte's experience tends to discredit such an opinion, and to prove that the maximum of density can be better attained by casting the metal under the proper conditions. At Terre-Noire the homogeneous cast steel has a density of from 7.8 to 7.9, which is seldom reached by forged steel. No doubt but that in special cases, and for certain shapes of metal, the old system will still have to be followed, but that the new, simpler, cheaper, and more efficient one will supersede it in most other instances.

HOMOGENEOUS STEEL.—M. Euverte, the present intelligent director of the works at Terre-Noire, read a highly important and most interesting paper to the Societe des Ingenieurs Civils, on the 4th of May last, on the subject of homogeneous steel, or steels without blows (*acier sans soufflures*), which completes in many particulars the paper presented to the Iron and Steel Institute at the last London meeting by M. Gautier, of Paris. This document enters so fully into details, giving the results of a series of long continued and carefully conducted original experiments, that we regret that we are not able to re-produce the whole of it. We commend it to the special notice of the steel manufacturing community, confining ourselves to the summary of conclusions, which, we believe, have an important bearing on the future of this special branch of industry.

The question of homogeneous cast steel is not a new one, and took rise the very day when it became possible to produce large bodies of molten steel. The attainment of the desideratum, which consisted in being able to furnish castings superior in resistance to iron —nearly equal in fact to forged steel—was, indeed very attractive. The Germans were the first to take the matter up, and, as early as the year 1855, specimens of their manufacture were shown on exhibition. M. F. Krupp came forward with ingots of a gradually increasing size, and Bochum exhibited moulded specimens, among which solid wheels were especially conspicuous. Neither of these said a word about the physical properties, nor in regard to the chemical constitution of the metal, which was exciting much admiration, and they even took special care to mention nothing as to the mode of manufacture. It was, however, soon known that silicium had something to do with the results obtained. Mr. Bessemer, as early as 1861, took out a patent for its application. About this date, however, several English scientific men, among whom we find Fairbairn and Prof. Riley, if we are not mistaken, indicated the properties of silicium in combination with iron. Until the present period the practical details of the process remained a profound mystery, the general opinion being prevalent that it was practically impossible to cast homogeneous steel otherwise than very hard, and deficient in those ductile properties which make ordinary steel so valuable.

RAILWAY NOTES.

ALLOWING FOR RAIL EXPANSION.—This is a point in practical railway building which, we see, was quite thoroughly discussed at the late meeting of the Master Mechanics' Association. In a paper by W. S. Huntington, the following table is introduced:

Length of rails, feet.	No. joints in a mile, one side of track.	Space for expansion, inch.	Fractions of an inch decimally expressed.			
15	352	.1026	1-64	.01562	1 2	.5000
16	330	.1090	1-32	.03125	9-16	.5625
18	293	.1228	1-16	.0625	5-8	.6250
20	264	.1863	1-8	.1250	11-16	.6875
21	251	.1434	3-16	.1875	3-4	.7500
28	188	.1914	1-4	.2500	13-16	.8125
30	176	.2044	5-16	.3125	7-8	.8750
36	146	.2465	3-8	.3750	15-16	.9375
40	132	.2725	7-16	.4375	1	1.0000

Among the reasons why it is best to be particular to have the rails the right distance apart at the joints are: If there is not room enough for expansion, the rails will bend to the form of a loop, causing death and destruction. And even if the compression is not sufficient to cause this, the effect on the traffic is destructive and causes breakages. If you are "out on the track" on a hot day, and the rails are "uncomfortably tight," and no trains in sight, you will be warned by the approach of one by the groaning and laboring of the track, as though it were a thing of life, and undergoing the most excruciating torture, or laboring like a ship in a storm at sea. The train may be two or three miles off, and out of sight, but you know it is coming by the cracking and snapping of the joints, as now and then a rail finds a little space and is thrust against its neighbor like a blow from a sledge. With this excessive compression on the rails and fish-bars, and the heavy rolling weight they are subjected to, the rails are strained and worked like a piece of tin bent back and forth between the thumb and fingers; and if there is a flaw or a weak spot it will soon amount to a crack and then break.—*Railway World.*

THE final result of railway working in 1876, as shown in the several reports to the Board of Trade on capital, traffic, and working expenses of the railways of the United Kingdom, by Messrs. Calcraft and Giffen, may be stated briefly as follows:—The extent of the system has been increased very little during the year, only 1.3 per cent.; but the double mileage has increased 3 per cent., showing the conversion of single into double lines. The capital at the same time has increasd 4.4 per cent., but part of the increase is in nominal capital only; and the capital per mile open has increased 3.1 per cent. The ordinary capital has, however, increased more slowly than the total capital, or only 2.9 per cent. At the same time the traffic has increased 1.6 per cent., or rather less than the rate of increase of capital; but the working expenditure has increased more slowly still, or only 0.9 per cent.; so that the increase of real earnings is 2.4 per cent., more nearly approaching the rate of increase of capital. The receipts, expenditure, and net earnings per train mile have all decreased slightly. The result is (1) a slight diminution of the percentage of net earnings on the whole capital—viz., from 4.45 to 4.36 per cent., a reduction which would be still less in reality to those concerned, allowing for the increase in nominal capital only as distinguished from an increase in actual capital outlay; and (2) a rather larger but still not a large diminution of the dividend paid on the ordinary capital—viz., from 4.72 to 4.52 per cent., a diminution which would also be less if allowance were made for the infusion of merely nominal capital. These are the results in a year in which the increase of traffic has been at a lower rate than at any time since 1858, the average rate having been in that period 5.26 per cent., while last year it was only 1.59 per cent. They are also the results at a time when the rate of working expenses is at a high level compared with the whole period prior to 1872. The result to railway capitalists in the circumstances cannot be deemed unfavorable on the average, though the average is, no doubt, composed in part of some unfavorable extremes. As regards the public use of railways, the facts stated as to the increase of third-class traffic, as well as of minerals and goods conveyed, would appear to show that use has been increased in 1876 in a greater degree than the return to the owners of the railway system.—*Engineer.*

MR. J. Rowland, Division Engineer of the Philadelphia and Atlantic City Railway, writes to the *Railroad Gazette*:—"It may be interesting to your professional readers to see this report of the most rapid railway construction on record. The Philadelphia and Atlantic City Railway is fifty-five miles in length. Ground was broken about April 1, 1877, and the first train was run over the road July 7. The deepest cut is 30 feet; deepest fill, 35 feet; largest single excavation, 40,000 cubic yards; embankment, 30,000 cubic yards. An embankment of 23,000 cubic yards, 1800 feet long, was made in one week from date of its commencement. The methods of construction are original with the officers of the road, and are of considerable interest to the profession. Five miles of track were laid in one day. There are over one hundred bridges and culverts; aggregate length of three longest, 1300 feet, one of them with 100 feet draw. The trial trip was made July 7, one and three-quarters miles of track being laid on that day in advance of the train, delaying it two hours; the road has 44,000 feet of wharf in Camden, built in two months, and has for its depots, offices and excursion house, Centennial buildings, known as Centennial Commissioners Building, La Fayette Restaurant, Board of Finance Building, Centennial Bank Building, Car Annexe to Machinery Hall. The equipment of the road is, eight locomotives, forty-four passenger coaches, sixty freight cars. Gauge, 4½ feet. Average cost of grading, 10 cents per cubic yard of excavation and embankment. Total cost of construction and equipment, $770,000. The line is as yet incomplete, but when brought to grade and completed according to engineer's plan it will be equal to any road in the United States for safety, speed, and comfort. T. F. Wurts, consulting engineer, constructed the road and organized the train force. Eight trains a day—four of them opposed—were run under his government, before the erection of telegraph line, at a speed of twenty miles per hour and with no great detention. The telegraph line is now in working order and superintendent appointed. Trains run through in two hours with no stops. A single train has carried 2300 passengers each way. When we see trains of twenty-nine passenger cars running, at a speed of thirty miles per hour, over a line of fifty-five miles in length which was commenced less than four months ago, the rapid stride of railroad progress is manifest. The engineer corps were employed night and day, sixteen hours being an average day's work for the consulting engineer."

ENGINEERING STRUCTURES.

BRIDGE BUILDING IN INDIA.—The erection of the new bridge at Broach has been entrusted to Messrs. Thos. White & Co., the builders of the Government bridge over the Taptee. They in this contract are partners with Mr. G. H. Bayley, the engineer, who it will be remembered, so ably superintended the construction of the temporary bridge at Broach. Mr. Bayley resigns his position on the Bombay, Baroda, and Central Indian Railway in consequence of sharing in this contract. The estimate for the erection of the bridge is £ 40,000 less than the estimate of Sir John Hawkshaw. The Government seemed at first very anxious that the work should be done by a home firm, and it is due to the persistent representations of Mr. F. Mathew, the agent of the B. B. and C. I. Railway, that the Government were induced to permit the contract to be tendered for here. We congratulate the company on their success; and when it is remembered that the work will be done chiefly by the excellent staff which erected the present temporary bridge so rapidly and so well, under the able superintendence of Mr. Bayley, there can be no doubt that this splendid bridge, will be completed within the prescribed time and in a highly creditable way.—*Times of India.*

A CENTRAL NEWS TELEGRAM SAYS:—Operations connected with the submarine tunnel have already been commenced on the other side of the Channel, several pits having been sunk to the depth of about 110 yards. At the same time the French and English committees have definitely drawn up the conditions of working for the route. The property of the tunnel is to be divided in half by the length, that is to say, each company will possess half of the line, reckoning the distance from coast to coast at low tide. Each company will cover the expenses of its own portion. The general work of execution will be done on the one side by the Great Northern of France, and on the other by the London, Chatham, and Dover and South Eastern Railway Companies. All the materials of the French and English lines will pass through the tunnel in order to prevent unnecessary expense and the delay of transhipment; as in France and in England railway companies use each other's lines, and goods can pass from one line on another without changing vans. It is understood that an arrangement will be established for a similar exchange of lines between all the English and Continental railway companies. When the tunnel is completed the tunnel will belong to its founders, but at the expiration of thirty years the two Governments will be able to take possession of it upon certain conditions.

SUBMARINE CABLES.—Particulars of the submarine cables now in existence have been stated as follows by the Director of the Bureau International, at Berne, Switzerland. Private companies—Number of cables, 149; miles of cable, 59,547; miles of wire, 65,535. Government telegraphs—Number of cables, 420; miles of cable, 4,442; miles of wire, 5,725. Totals, 569; 63,989; 71,262. The last total shows the extent of active wire as distinguished from the mileage of cable, some of them bearing as many as seven conductors. These figures give an average length of 112 miles for each cable, the average of the Governments being under 11, and that of the private corporations close upon 400 miles. The enumeration includes all wires laid in bays, gulfs, or estuaries on the coasts of the various countries, but excludes any laid across interior lakes or water courses. In the north of Europe, the Government system of Norway embraces no fewer than 193 cables, each carrying one wire and only making up an aggregate of 233 nautical miles in length. In Sweden, besides the joint possession with Germany and Denmark of submarine cables joining the Scandinavian Peninsula with the European systems, the Government has four cables, of an aggregate length of 22¼ nautical miles. Denmark has in its system twenty-nine different cables with a total length of 101 nautical miles. Holland possesses in all eighteen submarine cables, with an aggregate of 36 nautical miles, the longest being 16 miles. Although Russia possesses a telegraph second only in point of extent, in Europe, to that of the United Kingdom, the geographical formation of that vast empire is such that it only possesses three submarine cables, of 62 miles in all, besides its one-half share in the Crimea and Caucasus line. The cables of Germany in the North Sea and Baltic, and of Austria, in the Adriatic are forty-six in number, and 235½ nautical miles in extent. Turkey and Greece possess thirteen cables, of 147 miles in extent. Italy has twelve cables in all, one of them being 118 miles long. Spain has six, extending to 283 miles. The twenty-six cables of France are mostly short, though one of them—that from Marseilles to Algeria, 500 miles—is one of the longest in any national system. Great Britain with Ireland, has forty-nine cables, with a total length of 500¾ nautical miles, in addition to being owner of three cables to the Continent from the Norfolk coast, embracing 460 miles additional. Portugal is the only European nation which possesses no Government submarine telegraph.

The construction of most of the private lines has been due to British capital and enterprise. The Anglo-American Company has the longest submarine cable in the world (Brest to St. Pierre, 2,585¼ nautical miles), and a group of five Atlantic cables in all; yet the total length of its cables is exceeded by the system of the Eastern Telegraph Company. The seventeen cables of the former extend in all to 12,315 nautical miles, the longest of the subsidiary lines, used to join those from Europe at the insular shore ends on this side, being that from St. Pierre to Duxbury, Massachusetts, with a length of 749 miles. The Eastern Company has, including the Eastern extension, a total of 21,883 nautical miles, being about one-third of the whole existing in the world. The original enterprise presented a total of thirty-nine cables and 14,502¾ nautical miles, and the extension nine cables and 7,381 miles. Next in point of extent are the systems of the West Indian and the Brazilian Companies, the former having nineteen cables with a total of 3,970 miles, and

the latter three cables and 3,866 miles. If the landings at Madeira and Cape Verde Islands are left out of view, the line from Portugal to Brazil may be looked on as the longest cable in the world. These two systems—the West Indian and Brazilian—are united by the lines of the Western and Brazilian Companies with nine cables of a total length of 3,750 nautical miles. Next to the French-Atlantic cable the longest unbroken line of submarine wire is the cable of the Direct United States Company, from Ballinskelig's Bay, County Kerry, Ireland, to Tor Bay in Nova Scotia, which is 2,420 nautical miles in length.—*Iron*.

ORDNANCE AND NAVAL.

THE QUICKEST PASSAGE ON RECORD BETWEEN ENGLAND AND AUSTRALIA.—The *Melbourne Argus*, Sept. 3d, says—"The fastest passage on record from London to Melbourne has been made by the Lusitania, of the Orient Line. She arrived on the 8th ult., bringing English papers of three weeks' later date than those of the previous mail. The voyage has been performed in 40 days 6½ hours, inclusive of a detention of one day and seven hours at St. Vincent, where a call was made for coal, and the total time the steamer was under way was 38 days 23 hours and forty minutes. Some very fast steaming was done, and the average speed per day was 311½ miles, the greatest day's work being 344 miles. The Lusitania brought out 345 passengers, 68 of these being in the saloon. This quick passage of the Lusitania has excited much attention, showing, as it has done, that in the matter of steam communication *via* the Cape the colony may be better served by competition than by the subsidy of any particular line. The Lusitania goes home by the Suez Canal, which route has also been chosen for the homeward voyage of the Whampoa. It is interesting to note that, although the August mail was delivered in Melbourne a week before contract time, the time occupied between London and Melbourne was only one day less than the direct voyage of the Lusitania."

ARMOR PLATES.—Recently an armor-plate 16 feet long, 3 feet 6 inches broad, and 8 inches thick, and weighing 7 tons 16 cwt., was tested on board the Nettle, target ship, in Porchester Creek, Portsmouth, under the superintendence of Captain Herbert, of the gunnery ship Excellent. The plate was selected from the works of Messrs. C. Cammell & Co., Sheffield, as a sample of a number which have been manufactured by the firm for the turret ships Agamemnon and Ajax, at present in course of construction at Chatham and Pembroke. It is the invariable rule with the Admiralty, previous to accepting armor-plates from the manufacturers, to select a plate at random from a batch, and submit it to a practical test under fire. For the purposes of the trial the sample plate is bolted against a wood backing, which is carefully "dubbed" to fit the shape of the iron. It is then fired at from a short range, the shots being placed as close together as possible, the freedom, or comparative freedom, of the intervening ridges from cracks determining the character of the plate. Should it pass the ordeal, as A 1 or A 2, the batch are accepted, but if of a lower order of merit, they are always rejected. The customary test consists of four shots, but in the present instance it was of a more crucial character. It consisted of nine spherical projectiles from the smooth-bore 68-pounder gun, the charge being 13 lbs., and the range 30 feet. All the shots fell within a space measuring 31½ inches by 27½ inches, and overlapped each other. The indentations ranged from 1.44 to 1.97 inches. After the fourth shot a slight crack appeared at the junction of Nos. 1 and 2, and subsequently a few other hair cracks appeared, but none were produced beyond the shot indents, and these were all insignificant. The iron exhibited great ductility under the racking test, and the plate on examination proved in every respect satisfactory. The following are the results of the nine rounds:

Indentations produced by		Diameter of impact.
1st	1.72 inches	10 inches.
2d	1.44 "	9½ "
3d	1.62 "	10 "
4th	1.56 "	10 "
5th	1.4 " in center to left	9½ "
6th	1.88 "	10 "
7th	1.9 "	10 "
8th	1.88 "	10 "
9th	1.97 "	10 "

A few days ago it was stated in *The Times* that Messrs. Cammell were about to conduct a series of experiments with steel plates on behalf of the Admiralty. These experiments are likely to prove of the very greatest importance, and will probably exhaust the subject so far as steel by itself, or in combination with wrought iron, can be applied to the protection of our ships of war. They will, therefore, naturally excite much interest among naval and military men. The experiments grew out of the recent trials of armor targets at La Spezzia, and the plates are being constructed for the purpose of ascertaining how far steel, or steel combined with iron, is better adapted for armor than the ordinary plates now adopted in the Navy. As the experimental armor is only 9 inches in thickness, the coming tests can be looked upon only as of a tentative character; but should the results obtained be sufficiently encouraging to proceed further, it is intended to manufacture both larger and thicker plates, and to submit them to the impact of heavier ordnance. The test plates, which are now being constructed at the Cyclops Works, are five in number, and are of the uniform dimensions of 10 feet long by 8 feet wide. Each plate is 9 inches thick and weighs nearly 13 tons. No. 1 is of wrought iron, the same as hitherto used, and will serve as a basis of comparison. No 2 is of steel and, if possible, will be specially made of a quality and temper to resist shot without "starring" or cracking at a point distant from the part struck by the projectile. No. 3 is formed of "high" or hard steel, fused between two plates of wrought iron when the inside sur-

faces of the latter have been wrought by means of a specially constructed furnace to a welding heat. By this process a perfect union or weld between the steel and the iron is obtained. In a plate of this kind the hardest steel may be used, while the effects of starring may be reduced to the lowest limit of the protection which is afforded by the exterior surfaces of iron. No. 4 plate is formed of "high" steel between two "low" steel plates. The section will be like No. 3, but, instead of the two outside surfaces being iron, they will be made of a steel possessing the least possible amount of carbon. The above plates will be first tested, and will be afterwards followed by No. 5. This will consist of "high" steel, the surfaces having decarbonised, so that instead of steel, the surfaces will, to a certain extent, have become pure wrought iron. The great object sought to be attained in all these experiments is a material so hard that it will resist penetration without "starring" or breaking up—such a material as will, in fact, keep out or break up the shot without serious damage to itself. In addition to the above experiments there will be two new systems of belt fastenings tried at the same time. All the experiments will be made at Portsmouth, the 7 inch rifled Woolwich gun being used against the plates on the occasion at a range of 30 feet.—*London Times.*

AVALANCHE AND FOREST COLLISION.—*September* 11th, 1877.—The *Avalanche* was an iron sailing ship, full rigged; she was 1,161 tons, built at Aberdeen, in 1874, registered in London, and classed 100 A1. The *Forest* was a wooden, soft wood ship, of 1,422 tons, built at Hansport, Nova Scotia, in 1873, registered at Windsor, Nova Scotia, and classed A1. 10. Neither ship had been surveyed or certified by the Board of Trade.

The *Avalanche* had sailed from London, bound for Wellington, New Zealand, with a crew of thirty-four hands all told, having on board fifty-nine passengers, and a general cargo. She was provided with five boats, capable of saving 130 persons, two of them being life-boats. Three of the five boats were at davits and two on skids. She had two sets (whatever that may mean) of life-belts, and five life-buoys.

The *Forest* had sailed from London, bound to New York for orders. She was in ballast. She had a crew of 21 hands and not any passengers. She had three boats, one of them being a long boat, and the other two being galleys. The long boat was lashed keel downwards on the poop, and the two galleys were lashed keel upwards on the forecastle.

These two outward bound ships were both proceeding down Channel, and, at about 9 p.m., on the 11th September, 1877, were about fifteen miles S.W. of Portland. The night was not such as to prevent lights from being seen at a good distance; the wind was blowing a gale, force 8.

The *Avalanche* was, according to the report of the Wreck Commissioner, under foretopsails, foresail, single-reefed mainsail, inner and outer jibs, foretopmast staysail, and spanker. Of these, the mainsail only was reefed. The speed of the *Avalanche* was then from six to seven knots. The *Forest* was under reefed topsails, whole foresail, and foretopmast-staysail. Her spanker was not set. The speed of the *Forest* was then about three knots.

A collision followed; the *Forest* ran her bow into the port side of the *Avalanche* nearly amidships. The inquiry has brought out many interesting facts that do not bear on the question of collision, and, as regards that question, which is the main one, we do not for an instant suppose that the case will rest where it is. It was a settled belief, and a settled practice of the sea, that where one of two crossing ships is required by law to keep out of the way of the other, the other ship should hold on. It is just as much the duty of the one ship to hold on as it is for the other to keep clear. In the present case, the *Avalanche* being the ship which had to get out of the way, tried to pass under the stern of the *Forest*, and the *Forest* being the ship required by the rules to keep her course, so altered her course as to run full tilt into, what was before the collision, the further side of the *Avalanche*. If the decision of the Wreck Court means that when a ship which is required to hold on, and is expected to hold on, does *not* hold on, and thereby brings about a collision, the blame is to be shared by the other ship which is trying to pass a-stern, the mind of the mariner will be in doubt, and this where no doubt existed before. We think the decision does create this doubt.

The next practical lesson we may learn from this collision is the utter uselessness of life-boats and life-saving gear in cases of emergency. In our articles on the loss of the *Atlantic*, and on the *Loch Earn Ville du Havre* collision, we wrote at full length on this topic, and would refer our readers to the remarks we then made. The loss of the *Avalanche* again proves that all the array of boats and life saving gear now required by law to be carried, are a delusion in a sudden emergency, and that to do as some hair-brained people wish, that is to say, to require large emigrant ships to carry still more boats, would be to add to danger by cumbering the ship's decks. The life-boats and boats of the *Avalanche*, though hung in davits, were useless; while the common boats of the *Forest* stowed on the poop and forecastle saved some life. Still, the fact that two ships, having amongst them boats enough to carry three times the number of people imperilled, saved only as many as could be counted on the fingers, and that one only of the eight boats of the two ships got to land, is a point in favor of our argument against trusting to increased boat accommodation for saving life on emergency.

Another practical lesson, and one which our shipowners will not fail to avail themselves of, is, that the British notion of the number of seamen necessary to man a ship is extravagant. The Court expressed their opinion as follows (we quote from the *Times'* report):—"The Court was not disposed to blame the owners of the *Forest* for having on board their vessel a small number of seamen, although it was only at the rate of one man and a-half to every

hundred tons." The Assessors had advised the Court "that American vessels can be worked with fewer hands than English vessels, because they have lighter ropes and larger blocks."

If, therefore, British shipowners will put lighter ropes and larger blocks on board, what is to prevent them from working their ships with reduced crews, after the manner of Nova Scotian soft wood ships? But is it a fact that vessels of the class of the *Forest* are so superior in their running gear, and tackles and ropes and blocks, and in their appliances for working ships, to British clipper vessels of the *Avalanche* class? There was a time when Yankee ships were a-head of British ships in all "notions" for working economically, and with that epoch, we imagine, must the advisers of the Court have been fully acquainted. It is, however, something quite new to those, like ourselves, who are posted up to the latest inventions and improvements in shipping, to hear that in respect of ropes and blocks, and appliances for saving labor, a Nova Scotian soft wood ship is a-head of English clipper ships. In no *other* respect does the one come near the other; but if the clear advantage of a reduction of a man, or a man and a-half per 100 tons is to be obtained, it is not likely that they will remain behind in this respect.

Another point is that both ships were classed ships, the *Avalanche* 100 A1., and the other A1. 10; and yet the highly-classed iron ship, in despite of her bulkheads, went down with every soul but three. This, again, shows what we have all along said: that lives are lost in classed ships. It may be thought to be absurd to make this observation in the present case, but it is not, for the following reason: when Mr. Plimsoll endeavoured to compel Parliament to adopt and enforce compulsory classification of all ships as a nostrum to prevent all shipwrecks, he used to parade the whole number of lives lost at sea, including those lost by collisions and strandings, and we think it only right to remind our readers of this now, and by quoting this terrible case of the *Avalanche* while it is fresh in mind, to show how utterly misleading were so-called facts brought forward to enforce the necessity for compu'sory classification.

The last practical lesson to be learnt is the extreme endurance under certain circumstances of a soft wooden hull. The hull of the *Forest* floating bottom up defied the combined efforts of an ironclad ship and a number of gunboats. She was shot at with a heavy gun, she endured repeated explosions of several hundreds of pounds of gunpowder, and was at last towed in, much shattered it is true, to the Portland breakwater. It is only fair to assume that a foreign ironclad and foreign gunboats and their crews could not have made a greater impression than was made on her by Her Majesty's officers, seamen, gunners, and torpedoists. It took them many days to dislodge and then not to destroy the hull of the *Forest*, and the most valuable lesson is that if we wish to defend the mouths of our rivers from hostile attacks, the Government has but to lay its hand on all the soft wood ships in port and anchor a sufficient number bottom upwards at the various approaches.

BOOK NOTICES.

FORMULÆ FOR THE CALCULATION OF RAILROAD EXCAVATION AND EMBANKMENT. By JOHN WOODBRIDGE DAVIS, C. E. New York: Gilliss Brothers. For sale by D. Van Nostrand. Price $1 50

This is a new edition of a good work on Excavation and Embankment Computation. It has already been somewhat widely used and as widely approved.

The method is simple, exact and easily learned by the average student. For the purpose of instruction, or for a practical guide to use in the field, this treatise, for reason of its compactness and the completeness of its demonstrations, may be regarded as a welcome addition to this already well supplied department of technical literature.

THE WORKSHOP. New York: E. Steiger. For sale by D. Van Nostrand.

No. 12 of this periodical is at hand. It maintains the high degree of excellence which has rendered it so widely acceptable to lovers of art whether students or artisans.

Among the more striking illustrations in the present issue we notice a Writing Table, Bronze Candelabra from the Opera House in Paris, and a Tea-Table Cover. The latter is printed in colors.

STATISTICS OF MINES AND MINING. Eighth Annual Report of ROSSITER W. RAYMOND, Ph. D., U. S. Commissioner of Mining Statistics. Washington: Government Printing Office. For sale by D. Van Nostrand.

The body of this Report is in three parts. Part I is in ten chapters devoted severally to the mining industries of the Western States and Territories; Part II describes in four chapters metallurgical processes; Part III describes mechanical operations belonging to metallurgy.

It is to be regretted that this report ends the labors of the Commissioner.

No public documents, we believe, have ever issued from the departments at Washington which afforded so much valuable information in proportion to the cost of obtaining and presenting it.

LINEAR PERSPECTIVE AND MODEL DRAWING. London: Macmillan & Co. For sale by D. Van Nostrand.

This is a brief but complete treatise on linear perspective. The subject is presented in thirty illustrated lessons, in the first four of which the learner is supposed to draw on the vertical plane, and thereafter is introduced by easy stages to the higher problems.

MINUTES OF THE PROCEEDINGS OF THE INSTITUTION OF CIVIL ENGINEERS.

The published papers of the Institution received since our last issue are:

The Rajpootna (State) Railway, by Horace Bell, M.I.C.E.

The South Reserve Piers and Floating Land-

ing-Stage at Birkenhead, by Chas. Graham Smith.

The first is briefly a report of the first of the Indian Narrow-Gauge Railways. The gauge is one meter, 400 miles are completed.

Mr. Smith's paper, though short, is fully illustrated, and is a valuable addition to engineering literature.

SCIENCE SERIES, No. 33.

MECHANICS OF VENTILATION. By GEORGE W. RAFTER, Civil Engineer. New York: D. Van Nostrand. Price 50 cts.

This treatise has appeared in full in the pages of this Magazine. It is thoroughly practical in its character, and is furthermore a scientific presentation of the principles governing this important branch of sanitary engineering.

The public need yet to be instructed in the methods of securing health in the household, or at least securing immunity from those numerous ills which arise from bad ventilation and bad drainage. Contrary to the popular impression something more is required than knowledge of the bare fact that fresh air is desirable. It is yet to be generally learned that scientific skill is required to ventilate dwellings to the proper limit.

This little book proves this last statement in a concise and readable way.

WATER SUPPLY OF SOUTH AFRICA AND FACILITIES FOR STORAGE OF IT. By JOHN CROUMBIE BROWN, LL.D. Edinburgh: Oliver & Boyd. For sale by D. Van Nostrand.

The Hydrology of South Africa by the same author, a notice of which appeared in our pages last year, gave a full account of the changes in the climate of this region with suggestions for remedy of the present aridity. The present work is designed to show that in many places in the colony where protracted drought affects the inhabitants a supply of water is at command sufficient for all ordinary wants.

The work is divided into Four Parts, and each Part into several Chapters.

Part I treats of Meterology generally.

Part II is devoted to the Sources of Water Supply available for agricultural purposes in the Colony of Cape of Good Hope, and beyond.

Part III treats of the Supply of Water and facilities for Storage along the Western Coast of the Colony of Cape of Good Hope.

Part IV. Supply of water and facilities for Storage in Colonised lands adjacent to the afore-mentioned Colony.

The work evinces wide and careful reading on the part of the author, and although a large portion is chiefly of local interest, still a large part may be profitably read for the instruction afforded in Meterology.

But districts, everywhere, suffering or likely to suffer from short supply of water are in present need of the information so carefully compiled in this work.

MISCELLANEOUS.

FEW of the persons who handle Bank of England notes ever think of the amount of labor and ingenuity that is expended on their production. These notes are made from pure white linen cuttings only, never from rags that have been worn. They have been manufactured for nearly 200 years at the same spot—Laverstoke, in Hampshire, and by the same family—the Portals, who are descended from some French Protestant refugees. So carefully is the paper prepared that even the number of dips into the pulp made by each workman is registered on a dial by machinery, and the sheets are carefully counted and booked to each person through whose hands they pass. The printing is done by a most curious process in Mr. Coe's department within the Bank building. There is an elaborate arrangement for securing that no note shall be exactly like any other in existence. Consequently there never was a duplicate of a Bank of England note except by forgery. According to the *City Press* the stock of paid notes for seven years is about 94,000,000 in number, and they fill 18,000 boxes, which, if placed side by side, would reach three miles. The notes, placed in a pile, would be eight miles high; or, if joined end to end, would form a ribbon 15,000 miles long; their superficial extent is more than that of Hyde Park; their original value was over £3,000,000,000; and their weight over 112 tons.

THE DECOMPOSITION THEORY OF STEAM BOILER EXPLOSIONS.—At the recent meeting of the American Academy of Sciences, an apparatus was shown at work, which proved that steam might be decomposed by simple heat into the constituent gases of water—oxygen and hydrogen. The heat employed was a little over ordinary redness, but did not reach whiteness. This experiment is of the highest value, as illustrating a possible cause of boiler explosions. The apparatus was very simple—a flask in which water was heated, a tube conveying the steam into a closed platinum crucible, where it was again heated by a spirit lamp, and a tube thence carrying the superheated steam and the liberated gases to an ordinary pneumatic trough, where the mixed gases were collected in a test tube, while the steam was absorbed. At the conclusion of the experiment, the gases thus collected were exploded by a lighted match, showing beyond question that they were the components of water. The experiment indicates that this explosive mixture of gases may be formed in a steam boiler. But it can only result from the most culpable carelessness. The boiler must, at least in part, be raised to a full red heat. Then cold water must be injected, for so long as steam and the gases are mixed, the latter cannot explode. The injection of water must condense the steam in the boiler before it cools the red-hot iron. All these conditions being fulfilled an explosion of the gases may take place.

PRESERVING FENCE-POSTS.—On this subject the *Journal of Forestry* has the following: The proper seasoning of timber before being used in any structure is far more important than the season of the year it is felled in, kind of timber used, or preventives employed.

There are paints, washes, and heterogeneous steeps recommended for preserving posts, but each are comparatively costly, and only partially successful. One great objection to the application of solutions externally rests on the fact that the sap being confined accelerates decomposition in the interior. Most foresters must have observed this. What I would rocommend with fencing-posts is, the materials, when felled, to be directly sawn into posts and stored under sheds thoroughly ventilated, where they will remain at least a year exposed to "sun and wind." The neck or part between wind and water of each post should be slowly charred over a strong fire,—slowly, because our principle means heating the timber thoroughly to the heart, so as to extract any moisture which may be still lodged at the center, and hardening a crust on the surface of the posts. Afterwards, to prevent the posts absorbing water, they should be well coated with coal tar, having its acid destroyed with fresh quicklime. The tar should be thoroughly boiled to evaporate all watery matter, and applied boiling hot. A large tank, holding the posts set on end, and filled with the scalding tar from a boiler, answers the purpose very well. Of course, the upper half of the posts can be painted when placed *in situ*. I am fully convinced coal tar, properly applied to thoroughly seasoned timber, is far more effectual in preserving posts than creosoting poisoning, kyanising, or all the paraphernalia of iron prongs, sheet-iron wrappers (an American invention), &c. One great recommendation in favor of the above process is that it requires no skilled labor, and the cost is a mere trifle.

The Consumption of Smoke.—An address was delivered by Professor Osborne Reynold, F.R.S., at the inaugural meeting of the Manchester Scientific and Mechanical Society, the subject being the possibility of reducing the quantity of smoke and pernicious gases poured into the atmosphere. He said the ways in which this could be done might be summed up in three:—1. By the better burning of the coal and the purifying of the products from soot and sulphur; 2. The more economical use of coal, and hence the reduction of the quantity consumed; 3. The somewhat transcendental but much more complete method, if it is possible, of substituting some other power for that now derived from coal. With regard to the coal consumed under boilers, the general conclusion to which research had led was that although it was quite possible to completely consume coal in the furnace, yet to do so economically required constant attention and great care. In looking for further reforms, it was the small engines that were the difficulty, for not only did they fail properly to burn their own coal, but they prevented the adoption of measures which might be satisfactorily carried out where the consumption was large. It seemed, therefore, that the chance of further reducing the impurities turned into the air depended greatly on their ability to do away with small engines. This view raised another question, and a somewhat wide one—could they supplant small engines by power derived either from large engines or some other source? He did not doubt that if the useful work done by the warehouse and other engines in Manchester were measured it would be found to be less than one fifth what might well be obtained for the same coal. If, therefore, power in a convenient form could be obtained whenever and wherever required, at a fixed and reasonable charge, and with no other trouble than the throwing into gear of a clutch or the turning a tap, there could be no doubt that not only would it be applied in many instances where the inconvenience of a steam engine prevented its being used, but also it would be so largely used to supplant steam engines which were now kept working with little or nothing to do for the greater part of their time as to effect a considerable saving in the consumption of coal. He examined at length the only four ways in which power could be practically conveyed—namely, rotating shafts, endless belts or ropes, water in pipes, and air in pipes. He considered there was no hope for our ever utilising the natural sources of power, such as tidal rivers, for mechanical purposes, unless they conducted them on the banks of those rivers. But as regarded the substitution of a general source of power for the small steam engines now in use in our towns, the case appeared more hopeful; and, what was more, this had already been done in some instances. With the ability to have either water or air at the most convenient pressure and at a reasonable cost, he thought but few users of power on any but the largest scale would care for the trouble, danger, dirt, and expense of having steam engines of their own; and if this were so, there would then be a chance of reducing the impurities in the air.

Irrigation in India.—We are not a little pleased to find so eminent an authority as Sir Arthur Cotton holding opinions identical with those to which we have already given expression, concerning the relative value of irrigation and railways in Hindustan. Sir Arthur delivered an address to the Liverpool Chamber of Commerce on Wednesday, in which he said that his views were diametrically opposed in every important particular to those held by the officials of the India Office, both past and present. The Duke of Argyll, speaking on the subject not long ago, alluded to his—Sir Arthur's—opinions as mere visionary ideas, but he thought that was hardly the way to dismiss the opinions of one who, like himself, had had over forty years' experience as a civil engineer in India, and who had been engaged in the construction of two of the most extensive and successful irrigation works in that country. Sir Arthur proceeded to give the results of the irrigation works carried out up to the present time. The district of Tanjore, for instance, since the water had been introduced, had gone on in one uninterrupted chain of improvement. The revenue had increased from £430,000 a year to £750,000; the population was nearly doubled; and it was now, with only one exception, the most flourishing district in the whole of India. That exception was the district of Godavery. In 1846 this

district was in such a miserable state that the Government feared open rebellion, and he was sent down there to report, and to project works for the improvement of the land. They were five or six years in constructing the head works and main channels of the canal, and the continuation of the works had occupied from that time to the present. The Godavery district had increased two and a-half fold, and now was the most thriving district in India. In fact, the only three districts in the Madras Presidency where irrigation works had been adopted were now yielding a net profit to the Government of 15, 21, and 87 per cent. respectively. But in addition to the value of canals for irrigating purposes, they were available for transit; indeed, he had come to the conclusion of late years that cheap internal transit was even of more importance to the people than was irrigation, as by this means the whole interior of the country would be developed, and as far as the north-west provinces were concerned, all that fertile grain-producing country would be afforded cheap and easy communication with Calcutta, and so be enabled to supply the English market. At present this was impossible, owing to the high railway charges. The irrigation works so far carried out had cost on an average about £2 an acre, and the return on this in extra crops had been quite 100 per cent. Contrasting canal with railway works, Sir Arthur said that in Madras the Government had spent 14 millions on railways, and the return was about 2½ per cent.; but had this prevented the famine? In the same presidency there were three districts returning from 15 to 40 per cent. on the outlay for irrigation works, the land was overflowing with plenty, the people were prosperous, and immense quantities of grain were sent to the relief of the famine-stricken districts. In conclusion, he urged that if the Government would only develop the resources and revenues of India by means of a complete system of irrigation—for that he held to be the only really practical mode of improving the country—they would benefit both this country and India in many ways.—*Engineer.*

THE LONGEST TRUSS SPAN IN THE WORLD.—The Philadelphia *North American* says another solid achievement in bridge building and engineering has been made by the Keystone Bridge Company, in the successful completion of the longest span of truss bridge in the world. The bridge was built for the Cincinnati Southern Railway over the Ohio River. The first long span of truss bridge attempted in this country was the Ohio River bridge, at Steubenville, with a channel span of 320 feet. It was designed by J. H. Linville, and manufactured and erected under his supervision. Then followed spans of 350 feet at Parkersburg and Blair, of the Linville truss; then the 420-foot span at Cincinnati, and the great steel arch bridge at St. Louis with several spans, one of 515 feet, two of 500 feet, and others shorter. These were all erected by the Keystone Company. The new structure was tested on the 8th inst. Five of the spans comprising the bridge proper over the Ohio were built by the Keystone Company and five others on the Ohio side, over the streets and houses, by the Baltimore Bridge Company. The former was also awarded the contract for the eleventh span and the bridge over Eighth street. The structure as it stands cost nearly $700,000. Span No. 3 over the main current is 519 feet long—the longest truss span in the world. The bridge is built entirely of iron, except the cross-ties for the track and the guard rails. All the spans rest upon solid masonry piers, except the north end approach, which has iron piers with masonry bases. All the river piers are upon a rock foundation. Those upon the shore are upon a foundation of piles. The two piers supporting the long span are 110 and 119 feet high and 11 by 26 feet under the coping. The top of the rail is 105⅔ feet above extreme low water mark, and 43 feet above high water mark of 1832. The testing was performed with locomotives.

PROGRESS OF THE ST. GOTHARD TUNNEL.—The advancement made at the St. Gothard Tunnel during the last week in October, was 33 m. at the Gœschenen, and 17.9 at the Airolœnd—a progress of 7.2 m. per day. During all last week the work at the Airolœnd was interrupted by a terrible conflagration which almost completely destroyed the village.

The Federal council has addressed a circular to the governments of the cantons interested in the construction of the St. Gothard Railroad, calling their attention to the sensible progress made in the construction of the tunnel during the fifth year of the work, as compared with the preceding years. The circular says that, excepting unforseen obstacles, the tunnel will be completed in three years.

MR. W. HIGGINBOTTOM, the Mayor of Derby, wishes to make known the fact that the nucleus of a committee is forming there for the purpose of enabling the admirers of Mr. James Allport, the general manager of the Midland Railway, to combine in presenting him with a testimonial, in recognition of his public-spirited policy. Mr. Higginbottom suggests that the Lord Mayor of London should be prevailed upon to lend his assistance to the furtherance of this object, in which case his lordship's example would doubtless be followed by provincial mayors throughout the kingdom.

MR. THOMAS COLSON, C.E., who was specially sent out by Government to take charge of and complete the waterworks at Singapore, has returned to London, having accomplished his mission. For twenty years past efforts have from time to time been made to supply Singapore with water, but, owing to various causes, they have been unsuccessful. All difficulties, however, have been surmounted under the management of Mr. Colson, and that gentleman has been awarded much praise by the Singapore press for the ingenuity and skill displayed by him in his labors.

TWO patents for inventions by Mr. Wilde for the working of electric telegraphs have been extended. The patent granted in 1863 is extended for seven years, and a second patent, obtained in 1865, is prolonged for five years, so that both will expire together in the year 1884.